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SUPERCONDUCTING SUPER COLLIDER

HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON
ENERGY AND NATURAL RESOURCES
UNITED STATES SENATE

ONE HUNDREDTH CONGRESS

FIRST SESSION

ON THE

DEPARTMENT OF ENERGY'S FUNDING REQUEST FOR THE
SUPERCONDUCTING SUPER COLLIDER

APRIL 7, 1987

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SUPERCONDUCTING SUPER COLLIDER

TUESDAY, APRIL 7, 1987

U.S. SENATE,
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT,
COMMITTEE ON ENERGY AND NATURAL RESOURCES,
Washington, DC.

The subcommittee met, pursuant to notice, at 2 p.m., in room SD-366, Dirksen Senate Office Building, Hon. Wendell H. Ford, presiding.

OPENING STATEMENT OF HON. WENDELL H. FORD, A U.S. SENATOR FROM THE STATE OF KENTUCKY

Senator FORD. The whole Committee will come to order. Welcome to the Subcommittee's hearing on the Superconducting Super Collider.

The things we are going to hear today, about what would be studied in this facility, are about as esoteric as anything we have discussed in this Subcommittee since I have been in Congress. As I understand it, we will go from the smallest objects known in nature, the building blocks of matter, to the theories of why the universe is the way it is.

I am going to listen and hopefully learn.

We have some excellent teachers appearing as witnesses today, beginning with Dr. Trivelpiece. For many, many years, Al has been coming up here to patiently and carefully explain esoteric science to Members of Congress. He has always done a good job, even though he only has the likes of me to do it with.

He has also done a good job by the DOE research programs he has been responsible for. When he leaves this month to head the American Association for the Advancement of Science, DOE will be losing one of its most capable public servants, and basic science will lose one of its most effective advocates in the executive branch of the Federal Government.

The subject of today's hearing is the SSC, the grandest and most expensive fundamental physics project of this generation.

The Administration has proposed the project, but its future is going to be decided in Congress. This Committee will be involved.

The several billions of dollars that will be involved in building and operating the SSC will not come out of defense and will not come out of entitlements. We will be working with the discretionary part of the budget that is under immense pressure from the politics of reducing the deficit.

The money for the SSC will come as an addition to, or as a deduction from, the programs at DOE that we established in this

Committee. We have been able to keep most of the important programs alive, but at much lower levels of funding than they deserve.

The Administration says it wants to fund the SSC as an add-on to the budget. But the Administration also wants Congress to balance the budget. And the Administration is not above playing politics with Congress on the issue of balancing the budget.

So as I see it, something has to give. Those who want the SSC have their work cut out for them in my opinion.

With those remarks, we will be delighted to hear you, Doctor, and start your educational process.

Dr. TRIVELPIECE. Thank you, Mr. Chairman.

Senator FORD. Excuse me. Before you start, the Chairman is here.

Senator Johnston, did you care to make an opening statement?

STATEMENT OF HON. J. BENNETT JOHNSTON, A U.S. SENATOR FROM THE STATE OF LOUISIANA

The CHAIRMAN. Thank you, Mr. Chairman.

In one of my Appropriations Subcommittees, I have heard from Dr. Trivelpiece on this subject, and I wanted to say a word about the subject of the Superconducting Super Collider.

That is, that I strongly support it. I hope we will be able to find the money for it. I do not want to savage every other scientific program in our budget in order to fund it, and I'm hoping that we can find a source of funding for it because I think it is a very worthwhile thing.

But the advice I get from scientists in general is that they support it as well, but they do not want to see all the other programs in science savaged in order to support it. I have got a few programs I would not mind savaging in order to find that support—but I think the President may disagree with some of my plans.

So I am glad to have Dr. Trivelpiece here. I am sorry we are losing his services because I think he has done a really outstanding job and he is a great resource for science in the Administration.

So with that, I will not interrupt Dr. Trivelpiece any longer, except to welcome him to the Committee.

[The prepared statements of Senators Johnston, McClure, and Domenici follow:]

STATEMENT OF SENATOR J. BENNETT JOHNSTON

CHAIRMAN, COMMITTEE ON ENERGY AND NATURAL RESOURCES

HEARING ON THE SUPERCONDUCTING SUPER COLLIDER

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

APRIL 7, 1987

I AM VERY PLEASED THAT THE SUBCOMMITTEE IS HOLDING THIS HEARING ON THE SUPERCONDUCTING SUPER COLLIDER.

I TOO AM GOING TO LISTEN AND HOPE TO LEARN.

I ALSO WANT TO SECOND SENATOR FORD'S COMMENTS ON THE GOOD JOB THAT DR. AL TRIVELPIECE HAS DONE AS HEAD OF DOE'S ENERGY RESEARCH PROGRAMS. THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE IS GETTING A VERY GOOD MAN AND DOE IS LOSING A VERY GOOD ONE. WE WILL NEED TO REPLACE AL TRIVELPIECE AT THE LEVEL OF ABILITY HE HAS SHOWN.

THAT WON'T BE EASY.

WE ARE HERE TO TALK ABOUT THE SSC. FOR MANY PEOPLE THE ONLY ISSUE IS, "WHERE ARE YOU GOING TO SPEND THAT $5 BILLION - OR $6 BILLION OR $10 BILLION? CAN I GET IT IN MY STATE?"

I WOULD PUT IT TO YOU THAT THAT IS NOT THE ISSUE- NOT YET, ANYWAY.

THE ISSUE IS WHETHER THIS IS SOMETHING WE WANT TO DO AT ALL IN THE PRESENT BUDGETARY CLIMATE. AND IF THIS IS SOMETHING WE WANT TO DO IN SPITE OF THE PRESENT BUDGETARY CLIMATE, THE ISSUE IS "WHERE IS THE MONEY GOING TO COME FROM?"

I HAVEN'T HEARD ANYTHING THAT IS VERY CREATIVE ON THAT SUBJECT.

THE ADMINISTRATION SAYS JUST APPROPRIATE MORE MONEY.

BUT WHAT ARE THEY GOING TO SAY NEXT YEAR WHEN WE ARE CONSIDERING THE FY 1989 BUDGET IN THE SUBCOMMITTEE ON ENERGY AND WATER, WHICH I CHAIR, AND WE HAVE TO ADD $348 MILLION FOR SSC? ARE THEY GOING TO SUPPORT A $348 MILLION INCREASE OVER CURRENT SERVICES IN THE ENERGY AND WATER DEVELOPMENT ACCOUNT? I DOUBT IT.

MANY PROPONENTS OF THE SSC SAY "TAKE IT OUT OF DEFENSE, TAKE IT OUT OF SDI." BUT IF THIS PROJECT IS FUNDED AT THE EXPENSE OF OTHER PROGRAMS, IT WON'T BE AT THE EXPENSE OF DEFENSE, AND IT WON'T BE AT THE EXPENSE OF SDI. IT WILL BE AT THE EXPENSE OF ENERGY PROGRAMS AND AT THE EXPENSE OF OTHER PROGRAMS IN SCIENCE. THOSE ARE THE REALITIES. WE HAVE TO DEAL WITH THEM.

IF WE WANT THE SSC, WE BETTER REALLY WANT IT. WE BETTER WANT IT SO MUCH THAT WHEN THE SITE IS SELECTED IN SOMEONE ELSE'S STATE, WE STILL VOTE FOR THE APPROPRIATIONS, EVEN THOUGH THESE APPROPRIATIONS WILL BE VERY LARGE EVERY YEAR FROM NOW ON. WE BETTER WANT IT SO MUCH THAT WHEN THE HOME-TOWN PROGRAMS ARE HIT, WE STILL SUPPORT AND VOTE FOR THE APPROPRIATIONS FOR THE SSC.

SO AT THIS HEARING, LET'S NOT TALK ABOUT HOW TO GET THE SSC IN MY STATE. LET'S TALK ABOUT WHAT THE SSC IS, AND WHY IT IS SO VERY IMPORTANT.

LET'S SEE IF WE CAN BE CONVINCED THAT IT IS VERY, VERY IMPORTANT. BECAUSE THAT IS WHAT IT IS GOING TO TAKE.

OPENING STATEMENT

SENATOR JAMES A. McCLURE

APRIL 7, 1987, HEARING - SUPERCONDUCTING SUPER COLLIDER

Good afternoon. I am pleased that the Subcommittee on Energy R&D has scheduled this hearing on what appears to be one of the hottest scientific topics of discussion that the Congress and the Administration has seen for a long time.

After many years of discussion and debate within the physics community, followed by careful analysis within the Department of Energy, the decision to move forward with funding for the Superconducting Super Collider project was finally approved by President Reagan this January.

The ball is now in our court. Here in Congress, we are mindful of the widespread interest in this project by almost every State in the nation. We muse a the various nicknames, from desertron to potatotron, coined to personalize the project to the individual States who are seeking to have the SSC sited in their State.

If you read the news reports, you might be led to believe that Idaho is not a contender, lagging behind many of the other States. Idaho has dedicated $400,000 to the SSC while other States have committed upwards of $12 million. However, these figures fail to address the strengths of the Idaho National Engineering Laboratory (INEL) site--Idaho's proposed location.

The Land is already available--all 900 square miles of it--and it is FREE.

The INEL is already environmentally characterized, abundant water and ample electricity are available at relatively low rates.

Site support services already exist, as do facilities that include laboratories and office space.

The INEL has a well trained labor force, and excellent labor and community relations.

Idaho has a long and celebrated history of scientific and engineering achievements.

In Idaho and at the INEL, excellence has always been demonstrated and I would expect nothing less in the future. As you can plainly see--the SSC and Idaho would be a perfect match.

I think there is no question that a national commitment to the SSC would represent the largest single scientific undertaking since the inception of the NASA Apollo program. I have no doubt that such a commitment would go a long way to restore the leadership that the United States once commanded in the international scientific arena. It would send a clear and resounding signal to the world that America's respect for the frontiers of science has not waned.

I must, however, remark on some practical and hard-learned lessons of the past. Many of you who have served in Congress and on this Committee remember times in the past when large, challenging programs were conceived on the basis of vision, commitment, and the need. We learned from some of those experiences how fickle our hearts can be when courses of events--including changing needs and harsh fiscal constraints--cause us to recede from our original commitments. The Clinch River Breeder Reactor Project and the Synthetic Fuels Corporation being just two that immediately come to mind.

I only say this to remind everyone that our responsibility as an authorizing Committee can get the SSC proposal off the ground, but that is going to take a lot more than just this year's commitment or even next year's commitment to turn this proposal into a reality. It is going to take a firm and lasting commitment on the part of the entire nation for more than a decade.

If we accept this commitment, we will be giving our approval to the next logical step in particle physics research: building a facility that will allow scientists to look farther than ever before to the phenomena that created the universe. It is a staggering endeavor, with a potential for staggering results, at a staggering cost.

We must decide if it is a cost worth incurring. I mentioned the Apollo program earlier--a project that has given us any number of technological spinoffs. Everything from supercomputers on the one hand, to Tang on the other.

I have no doubt that the Supercollider project will create any number of technological spinoffs that will benefit society.

I think the Supercollider project will usher in a new "golden age" of American physics, where American scientists and others from around the world will come to unlock the secrets of matter. It may not have the high public profile of a moon shot, but accepting this commitment is just a lofty a goal.

OPENING STATEMENT

SENATOR PETE DOMENICI

APRIL 7, 1987, HEARING - SUPERCONDUCTING SUPER COLLIDER

· GOOD AFTERNOON. I WISH TO THANK SENATOR FORD FOR SCHEDULING THIS HEARING ON THE SUPERCONDUCTING SUPER COLLIDER.

THIS SUBCOMMITTEE IS NOT USED TO DEALING IN DETAIL WITH ISSUES AS ESOTERIC AS ATOM SMASHERS AND PARTICLE PHYSICS. BUT BELIEVE ME, THE RESPONSE OF THE STATES TO THE PRESIDENT'S ANNOUNCEMENT LAST JANUARY TO BUILD THE SSC CERTAINLY HAS OUR ATTENTION. AND THE BUDGET PROFILE OF THIS PROJECT OVER THE NEXT TEN YEARS OR SO CERTAINLY HAS OUR ATTENTION.

SO I GUESS WE OWE IT TO OURSELVES AND TO OUR CONSTITUTENTS TO SPEND SOME TIME EXPLORING THE PROJECT'S MERITS AND DELIBERATING ON THE DECISION WE MUST MAKE TO AUTHORIZE THE SSC.

WHILE I WOULD BE THE LAST PERSON TO CRITICIZE THE RATIONALE BEHIND THE SSC PROPOSAL, I WOULD BE ONE OF THE FIRST TO ADMIT A CERTAIN FRUSTRATION AS TO HOW THE CONGRESS SHOULD WRESTLE WITH THE FINANCING OF THIS COLLOSAL PROJECT. SPEAKING SOLELY FROM MY EXPERIENCE ON THE BUDGET COMMITTEE, I SEE A LOT OF GOOD PROPOSALS OUT THERE. I SEE RECOMMENDATIONS TO DOUBLE THE NATIONAL SCIENCE FOUNDATION BUDGET BY 1992. THE NASA SPACE STATION PROGRAM WILL COST OVER $16 BILLION. OUR COMMITMENT TO STUDY THE GLOBAL ENVIRONMENT AND THEN TO CLEAN IT UP WILL NOT COME CHEAPLY. I'M HEARING ALL KINDS OF RUMORS THAT WE'LL BE EMBARKING ON A $3 BILLION PROGRAM TO UNLOCK THE SECRETS OF THE HUMAN GENE. TOP THIS ALL OFF WITH OUR COMMITMENTS TO SDI RESEARCH, AND WE REALLY DO HAVE AN ENORMOUS STRAIN BEING PLACED ON THE BUDGET. AND THE BUDGET COMMITTEE IS DOING EVERYTHING IT CAN TO ACCOMODATE THESE

NEW INITIATIVES. IT REMAINS TO BE SEEN, HOWEVER, JUST WHAT THE APPROPRIATIONS COMMITTEE IS LIKELY TO DO.

I DON'T MEAN TO BELITTLE THE IMPORTANCE OF THE SSC PROJECT, FOR THERE WOULD UNDOUBTEDLY BE ENORMOUS BENEFITS ASSOCIATED WITH THE FACILITY AND WITH THE KINDS OF SCIENTIFIC BREAKTHROUGHS THAT IT WOULD GENERATE. I AM MERELY TRYING TO MAKE THE POINT THAT THERE ARE OTHER PROJECTS OUT THERE AND OTHER PEOPLE WITH OTHER VOTES ON THE MATTER, AND SO WE'LL NEED TO BE VERY CLEAR IN THE DECISIONS WE MAKE ON THIS PROJECT, AND THOSE DECISIONS MUST NOT BE MADE IN A VACUUM.

SO WITH THAT IN MIND, I AM ANXIOUS TO HEAR FROM THE DEPARTMENT AND FROM EXPERTS CLOSE TO THIS PROJECT, AND I HOPE THAT THEIR TESTIMONY AND COMMENTS WILL IMPART TO US A BETTER UNDERSTANDING, A BROADER PERSPECTIVE, AND A GREATER APPRECIATION FOR THE SUPERCONDUCTING SUPER COLLIDER PROJECT.

Senator FORD. Senator Hecht, did you have a statement you wanted to make?

Senator HECHT. No.

Are you leaving? I did not know anything about it.

Senator FORD. Do you all not communicate on your side? [Laughter.]

Senator HECHT. I thought something scientific is nonpartisan.

Senator FORD. It is when we have it.

Senator HECHT. It is nice to see you.

Senator FORD. Doctor, you may proceed.

STATEMENT OF ALVIN W. TRIVELPIECE, DIRECTOR, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY

Dr. TRIVELPIECE. I hope you will bear with me. There are a couple of things I'd like to get on the record in addition to my prepared statement.

First, I want to thank you for these very kind personal remarks. I appreciate very much what you said, and I appreciate the privilege of having appeared before this Committee. You've always been a strong supporter of the Department's activities, and it's greatly appreciated, both corporately and personally. Thank you.

The place where I would like to begin is to refer to a charge letter that I sent to the High Energy Physics Advisory Panel a few years ago, in which I asked them what is the most important next step for high energy physics. The report that they sent back and that charge letter I would like to have made a part of the record with your permission, and several other—not all this large stack here, but some of these smaller things.

[The letter follows. The report DOE/ER-0169 July 1983 has been retained in subcommittee files.]

Department of Energy
Washington, D.C. 20585

January 27, 1983

Professor Jack Sandweiss
Chairman, HEPAP
Department of Physics
Yale University
New Haven, CT 06520

Dear Jack,

The collection of people that have contributed to our improved understanding of the fundamental constituents of matter and the forces that influence their behavior have done a remarkable job. Over the years this group has unraveled one seemingly unfathomable mystery after another. At each stage of this process there has been a need to have some combination of tools, talent, financial resources, luck, and faith. In some cases, the "combination" has involved gifted insights by a single, or a few, individuals and in other cases, it has involved large teams, massive apparatus, and years of hard work.

As with most such endeavors that seek fundamental knowledge and are tax-supported, there is rather intense competition for limited resources. The evolution of high energy physics from many small research projects at universities to a few large-facility major research centers has not been without competition, debate, and the need to make difficult choices from among several almost equally desirable possibilities.

A certain amount of this competition is healthy, but it can also be destructive. I am concerned that the present debate on directions and distribution of resources within the high energy physics community is taking on some unhealthy characteristics that have the potential for making the situation worse for all concerned. I am only too well aware of the budgetary problems that this and other science programs are facing, and I am sympathetic with the frustrations that have led to the acrimony and quasi-lobbying efforts of some of the high energy physics community. However, these efforts are not helping to resolve the problems, and I would ask you, through HEPAP, to encourage a more restrained and statesmanlike approach by all members of the community.

The High Energy Physics Advisory Panel (HEPAP) provides an organized and open forum in which the community can debate important issues and reach a consensus on the best course of action to ensure that effective progress is made in the field. The Panel and its special subpanels are well respected in Washington for the way in which they have faced up to tough issues and provided sound advice. In that regard, I am requesting that you work together with Jim Leiss, the Associate Director for the Office of High Energy and Nuclear Physics, to develop a charge letter for the 1983 New Facilities Subpanel that makes it clear that what is needed at this time are unequivocal recommendations on the research programs that will, in their opinion, make the most effective progress. I then expect HEPAP to debate these recommendations and to endorse, qualify, or reject them, as might be appropriate.

Whatever the outcome of your deliberations, I hope that you will not deliver up "soft" recommendations at the end of this process. By that I mean that it will not be particularly useful to have recommendations that we could do thus and so if we just had so much funding. What are the most important high energy phyics facilities for the United States to have, regardless of where they might go or how much they might cost? What are the elements of a balanced high energy physics research program that will ensure continued progress in experiment, theory, and the associated advanced technology. These are the kinds of questions that the Subpanel and HEPAP will have to consider in making their recommendations.

I would like to discuss with you and Jim Leiss the Subpanel Charge which you develop, prior to its delivery.

Sincerely,

Alvin W. Trivelpiece
Director, Office of
Energy Research

Dr. **Trivelpiece**. What they said in that panel report was that: "The sub-panel unanimously recommends the immediate initiation of a multi-TEV high luminosity proton-proton collider project, with the goal of physics experiments at this facility at the earliest possible date."

So from my perspective, that established the point of view that we should at least try to decide whether or not this was worth doing. In order to do that, we set up what was called a central design group, and that's a group at Berkeley that was charged with the responsibility of trying to determine what the deliverables are, how long it's going to take to build it, and how much it's going to cost, and so on.

Without going into details, the output of that group is shown here in this stack on my left-hand side. That led to the conceptual design report.* The conceptual design report then identified what I said: how long it takes, how much it costs, and the fact that there are no particular technological hurdles. I certainly don't propose that those be included in the record.

These reports, when delivered to the Department in March—or April 1986, were reviewed by the Department. That review then led to this report, in which we carefully analyzed what their statements were. I believe this is one of the best analyzed projects that the Administration has ever proposed to the Congress—in terms of knowing exactly what it takes, how much it takes, how much it costs, and how long it's going to take.

The point of all this is that I wasn't going to proceed to recommend to the Secretary that we go forward with this project until I was satisfied that the science was in good shape and that the feasibility was acceptable.

After that, I began the process of briefing the Under Secretary and the Deputy Secretary and eventually Secretary Herrington on this project. Early in December Secretary Herrington made public the fact that he supported proceeding with this.

But the problem was, as you well know, that the SSC is going to cost $4.4 billion in 1988 dollars for construction, commissioning, detectors, and so on. The Secretary decided that, on that basis, this was really a policy issue and as such should be reviewed by the Domestic Policy Council.

So during the month of December there were two meetings with the Domestic Policy Council. The decision that came out of that was that this should be taken directly to the President. It was, and on January 29 I had the good fortune to be able to make the presentation to the President, along with Secretary Herrington. The next morning the President decided in favor of proceeding.

One of the things I want to point out is that, contained in the President's decision to proceed was the admonition or the understanding that we should seek the maximum possible cost sharing. That's from foreign governments, from state and local governments, and from the private sector.

With regard to the foreign governments, that process has been formally initiated. It is our intent to proceed through regular diplomatic channels, to determine what their interest might be and seek from them any proposals, and then see what it is that they would expect in return and try to come to some decision on whether or not that is a desirable situation for the United States.

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*DOE/ER-0267, Report of the DOE Review Committee on the Conceptual Design of the Superconducting Super Collider, May 1986.

The situation with respect to state and local governments is that whatever cost sharing contributions that they would wish to make to this activity, they would make known in the process of responding to the invitation for site proposal. I call your attention to the fact that at least the first milestone the program was supposed to make was to get the invitation for site proposal out on the first of April, and that was accomplished. And that's now in the hands of all those who wish to propose.

Regarding the cost sharing from the private sector, I think it's a little bit more difficult to assess how that would come about. Once the site selection has taken place, it's entirely possible then, through the bidding and the contract process of responding to opportunities for the various companies, that they will make contributions to this project.

Why does the Administration support proceeding with the SSC? There are a variety of reasons, but one of the main ones is that this is a continuation of a process that really began in ancient times. Early man had the relatively easy job of defining his universe in terms of four elements, which are earth, air, fire, and water. Then the Greeks came along in about 650 B.C. and complicated it somewhat by pointing out that there were probably small or indivisible objects which they called atoms.

Surprisingly, things didn't change very much until the nineteenth and twentieth century. In 1932, with the discovery of the neutron, there were then three sub-atomic objects: an electron, a proton, and a neutron. Some of the scientists of that period actually believed that that was the complete set, that there were no more fundamental properties of matter and that everybody could give up on this particular activity.

But during the thirties, there were also some other discoveries that took place, that there really were additional subatomic objects; and also a tool with which to explore was invented during the 1930's. E.O. Lawrence invented the Cyclotron at Berkeley, for which he received the Nobel Prize in 1939.

Well, with this new tool the hope was that the scientists working this field would discover that there were a few simple objects held together by a few simple forces, and that all this would make great sense. Unfortunately, it didn't prove to be the case.

It turned out that matter was extremely complex and complicated, and it wasn't until about 1963 that Murray Gill-Mann proposed that many of these sub-atomic objects were themselves composed of additional smaller sub-atomic objects, which he called quarks.

Well, we don't know what the end of the story is, and I'm not going to try to do what some of the scientists who follow me are going to be in a better position to do, and that is talk about the intellectual aspects of some of this.

The SSC is a tool that will make possible continued progress in trying to understand these things. From my point of view, these are some of the more interesting questions of our times; namely, what are we made of, how did all of this begin, and where is it all going? These are the kinds of things that make life very interesting.

I would like to call attention to something else I would like introduced into the record, and this is a brief report called "The Economic Utility Resulting from CERN Contracts."

[The report follows:]

CERN 84-14
11 December 1984

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
CERN EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

ECONOMIC UTILITY RESULTING FROM CERN CONTRACTS (SECOND STUDY)

M. Bianchi-Streit, N. Blackburne, R. Budde, H. Reitz,
B. Sagnell, H. Schmied*) and B. Schorr

GENEVA
1984

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*) University of Strasbourg, France.

ABSTRACT

The study attempts to quantify the economic benefit to high technology manufacturing industries involved in CERN contracts, in relation to their sales to CERN. It covers the period 1973-87 and complements an earlier study made in 1973-75 (see CERN Report 75-5). Interviews were carried out in 160 European firms, who supplied estimates of increased sales and cost savings due to CERN contracts. This "economic utility" totals 3107 million Swiss francs (up to the year 1987), compared to sales to CERN amounting to 748 million Swiss francs in 1982 prices. It is estimated that, by 1987, CERN's high technology purchases made in 1973-82 will have generated Economic Utility amounting to about 60% of the overall cost of the Organization during the same period. In 1982, about 75% of the increased turnover due to CERN resulted from sales to markets outside high energy particle physics, for example, railways, shipbuilding, refrigeration, power generation and distribution, and health care. The quantification model used is discussed in detail and some specific cases are presented as examples. The industrial managers interviewed during both studies have confirmed that the forecasts made ten years ago were, on average, accurate.

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SUMMARY

This report describes the methods and the main results of a study which aimed at the quantification of Secondary Economic Utility, defined as the sum of increased turnover and cost savings, generated in European high technology industry due to orders placed by CERN during the period 1973-82. The study was similar to one made ten years ago, covering the period 1955-78 (see Ref.[4]). However, based on the experience gained during the first study, a general quantification model and improved sampling and extrapolation methods were used this time.

Managers in 160 firms out of a total of 519 high technology suppliers to CERN were interviewed. They estimated their Economic Utility using the general quantification model presented to them during the interview. As in the first study they were asked to give, if possible, five-year utility forecasts; and it emerged during the study that when managers feel able to make forecasts, these are, on average, reasonably accurate.

If the utility obtained for the firms interviewed is extrapolated to the total of 519 high technology suppliers to CERN, the resulting total utility for the period 1973-87 expressed in 1982 prices amounts to 4800 millon Swiss francs (where the values for 1983-87 are based on forecasts). The extrapolation error is about ±600 million Swiss francs. During the period 1973-82 CERN's purchases from the 519 firms amounted to 1380 million Swiss francs.

It is estimated that about 15% of the Economic Utility generated since 1973 has come from purchases made prior to 1973. Correcting for this reduces the total Economic Utility to 4080 million Swiss francs for purchases made since 1973. The corrected utility/sales ratio is 3.0; which means that one Swiss franc spent by CERN in high technology generated three Swiss francs of Economic Utility. The overall cost of the Organization during 1973-82 was 6945 million Swiss francs, which gives a value of about 0.6 for the ratio of the corrected utility to total CERN cost. It may therefore be stated that, by 1987, CERN's high technology purchases made in 1973-82 will have generated Economic Utility amounting to about 60% of the overall cost of CERN during the same period.

A comparison of results obtained in the first study with those of this study indicates that CERN's "utility creating power" is essentially the same as ten years ago, and it also appears that the high technology branches supplying CERN are suffering less from the present economic difficulties than other sectors of industry.

CHAPTER 1

BACKGROUND TO THE STUDY

About 30 years ago, a number of European countries collaborated to establish CERN in order that Europe could regain its place in the forefront of particle physics research. CERN has since built a range of sophisticated accelerators and research equipment to fulfil its mandate. Considerable resources have been necessary to accomplish this, and CERN has in consequence found itself assuming a technological and economic role in Europe in addition to the important scientific results it has achieved.

Economic and cultural effects of fundamental sciences, and the processes by which they create direct and indirect benefits to the society, have been reviewed from time to time, Refs.[1] and [2], for example. The primary function of CERN is to carry out "very basic research" in particle physics, and the direct product this research work is increased scientific knowledge or "culture", which may in the long term give rise to innovation. An additional direct effect concerns CERN's educational role in the training of physicists and engineers. The quantification of the economic value of these direct effects would be extremely difficult, if not impossible. However, CERN buys large quantities of high technology equipment and the firms fulfilling these orders often observe short-term (several years) economic advantages as a result of collaboration with CERN.

A first study, made in 1973-1975, attempted to quantify these secondary economic effects generated by CERN. In this study about 130 firms which had contracts with CERN were interviewed and asked to quantify such effects. The study verified that CERN, with its purchases of high technology equipment, often causes positive changes in the turnover and the production techniques of its suppliers, and that managers are prepared to elaborate and communicate quantitative information related to these effects. Details of this first study are published in Refs.[3] and [4]. Similar results were obtained during studies which quantified secondary economic effects created by the European Space Agency, Refs.[5] and [6].

Since the time when the first study on the quantification of secondary economic effects resulting from CERN contracts was made, the methodology of quantifying the economic effects has been refined, Europe's economic situation and its technology have undergone significant changes, and CERN itself and its relationships with industry have changed. CERN now spreads its orders over a greater number of firms than it did in the past, the number of other European and national organizations buying high technology equipment has increased; and European industry has been able to close some of the technological gaps which existed 15-20 years ago between Europe and the United States. A study of the resulting change in the nature of the economic effects is clearly of interest. In addition, the first study took place at a time when the effects being measured had arisen mainly during the construction period of the Intersecting Storage Rings (ISR). The effects arising during the construction period of the Super Proton-Synchroton (SPS) were, however, only partly covered by forecast values given by industrial managers. For all these reasons, it was decided to make a second study.

This paper reports results of the second study, carried out between 1982 and 1984. Improved methods for the quantification of secondary economic effects, as compared with those used in the first study, have been applied; in particular, a general quantification model has been developed to achieve the quantification of these effects in a more systematic and rigorous way. In addition, greater efforts have been made to define the total family of suppliers of high technology equipment to CERN, and to obtain a representative sample of these suppliers. This allowed the extrapolation of the results obtained for the sample to the complete set of firms from which the sample was taken.

CHAPTER II

ECONOMIC EFFECTS OF RESEARCH CENTRES

II.1 GENERAL DESCRIPTION OF THE EFFECTS

The principal aim of research centres is the generation of new knowledge, part of which, depending on its nature, will be used sooner or later as input in the innovation process at the end of which stand new or improved products or services. Beyond this, however, research centres may have important economic effects within the whole network of the economy. These effects may be essentially divided into three categories.

i) PRIMARY ECONOMIC EFFECTS: These result from the primary aim of the research centre when the centres produce innovations themselves, such as new energy sources, telecommunications satellites, etc.

CERN's primary aim is very basic research in particle physics and practical applications of its results can rarely be foreseen. Therefore, economic effects resulting directly from research done at CERN will hardly occur in the foreseeable future. However, the importance of long-term basic research for future technological progress is well known and has been discussed in some detail by Lederman, Refs.[1] and [2], and in the IIT report, Ref.[7].

ii) SECONDARY ECONOMIC EFFECTS: It is generally found that a major part of the scientific equipment necessary for carrying out the research is supplied by industry. Often the specifications and requirements are beyond the "know-how" currently available and thus represent a challenge to the manufacturer. Positive effects, such as new products, quality improvements, productivity increases, etc., arising from this challenge may be called the "Secondary Economic Effects" of research institutes, Ref.[8]. They are also sometimes called "spin-off" or "fall-out".

The present CERN study is confined to these secondary economic effects. A quantification of all the secondary economic effects resulting from CERN is probably impossible. It was therefore necessary to select certain main effects which could be quantified, and for which industry was prepared to provide data. The description of these main effects, which we call "Economic Utility", is given in the next section.

iii) MULTIPLIER EFFECT: This occurs with all public investments that create additional demand for goods.

It is because of the multiplier effect that the direct spending of CERN's material budget in the member states, and the spending by CERN personnel in the region where they live, also stimulate the economy and create employment. These effects are not quantified in this study, but have been evaluated for DESY in Hamburg, Ref.[9]. The Geneva Statistical Office, Ref.[10], has studied the effects of the international organizations in Geneva.

II.2 ECONOMIC UTILITY

In order to fit the secondary economic effects into a larger framework we may use the "surplus" concept developed by Massé, Ref.[11], which is described in more detail in Appendix A. This concept considers all the economic agents (employees, shareholders, customers, suppliers and the state) in relation to the firm. It defines the gains, positive or negative, which these agents realize as a result of changes in industrial production. The sum of these gains, assuming it is positive, is called the "surplus" by Massé, who defines its value by comparing different years. But we can also define the surplus by comparing two different situations during the same time period. We can, for example, compare a firm which is in business contact with a research centre such as CERN, and the same firm without this contact. The comparison of these two situations may also show a gained surplus.

As shown in the Appendix A, the parts of the gained surplus, as reported by managers in firms which have been influenced by a research institute such as CERN, are increased turnover on the sales side and cost savings on the procedures and production side. We call the sum of these two effects "Economic Utility" so that

$$\text{Economic Utility} = \text{Increased Turnover} + \text{Cost Savings}.$$

It is this part of the secondary economic effects due to CERN which is quantified in this study, and which was also quantified during the first study.

CHAPTER III

ECONOMIC UTILITY RESULTING FROM CERN

III.1 THE ECONOMIC UTILITY CREATION MECHANISM

Economic utility is stimulated by CERN through its purchases from industry. Indeed much of the equipment needed by CERN poses new problems, either by the technical specifications, or even by the sheer volume of complicated non-standard goods (e.g. hundreds of high-precision electromagnets or high-performance vacuum pumps) which are necessary for the building of an accelerator or related experimental equipment.

Firms which meet the challenge of such deliveries may afterwards be able to offer and sell new or improved goods to their other customers. Thus sales increases may be due to the following mechanisms:

i) NEW PRODUCTS: Firms may be led to market new or improved products as a consequence of the acquisition of new technology they have developed for, or together with, CERN. Also the volume of orders placed by the Organization, its non-competitive relation with industry, and the possibility of monitoring the performance of equipment delivered to CERN, may incite a firm to develop new products.

ii) MARKETING: The success of a product depends finally on its sales on the market. CERN's criteria for selecting products are known to be rigorous, and CERN is obliged to choose its suppliers on the basis of price for well-defined specifications. Considerable efforts are devoted to comparing products in order to choose the most suitable ones, and other customers may profit from this work by following CERN's choice. Further, material delivered to CERN is subject to strict performance control when in operation, sometimes for very long periods. Firms may refer to these facts in order to persuade other customers to buy their goods.

(It might be argued that a CERN supplier which increases its share of the market does so at the expense of its competitors; and that, from a European point of view, there might be no positive effect at all. This argument would be correct only in a market with conventional goods and a constant volume. We are dealing with markets which have high growth rates (electronics for example), and/or with products which change constantly due to rapid technological evolution. CERN buys products of the highest quality adapted to its purposes and at the lowest price. The resulting higher turnover forces the competitors of the CERN suppliers to improve their products, to the benefit of other customers. This is an efficient mechanism for keeping European industry abreast of overseas competition).

iii) QUALITY: Many firms producing in high technology fields sell their products primarily on the basis of quality. Contributions to quality represent, for these firms, an important element, since it permits them to maintain or improve their place in the market. CERN's quality requirements are often higher than that initially offered by the firms, but not so high as to lead to products which are overpriced and hence find no other customers. CERN therefore has an appreciable effect, not only as a "quality standard", but also through its capacity to help overcome technical problems when they arise.

iv) MAINTENANCE OF PRODUCTION CAPACITY: Some areas of high technology are still awaiting a breakthrough in the marketplace. In the meantime, orders from research centres, such as CERN, may assure the survival of some production capacity related to such areas, thereby contributing to Europe's technological independence. CERN orders may generally help firms to survive difficult periods when other orders are low.

Cost savings may result directly from the CERN-industry connection if, for example, a firm learns something new as a result of an order from CERN. This may occur by fulfilling the Organization's requirements, or through contact with its engineers who showed how to improve a particular production process, or by using research and development results from CERN. Indirect cost savings may result from increased sales to - or stimulated by - CERN as described above.

There are occasional losses resulting from the relation between CERN and industry, or as a side-effect of generating utility. These have to be taken into account when economic utility due to CERN is quantified. This fact is covered by the general quantification method which is described in the next section.

III.2 THE QUANTIFICATION METHOD

Economic utility caused by research establishments is actually produced in, and by, industry and this is where the data have to be collected for its quantification. They are in most cases, not readily available and are often confidential. Hence some conditions must be fulfilled in order to carry out a utility study successfully. First, the information must be collected by means of personal interviews. Many managers are prepared to provide information orally which they would not convey in writing. The mechanisms of utility creation are too complex to be covered entirely by a questionnaire, which could lead to misunderstandings. Second, quantification models have to be developed, which are sufficiently concise to be explained within the limited time available during the interview, and which are sufficiently powerful to cover the multitude of cases which occur.

Based on the experience gained in the first study, a general quantification formula for the quantification of economic utility due to CERN was developed for the new study. The basic ideas which underly the formula are given below.

For simplicity, the explanation of the basic ideas is limited to an example of the quantification of increased turnover utility. The general case including the quantification of cost savings utility is given in detail in Appendix B.

We assume that there are basic activities in a firm which essentially cause economic results such as turnover. Such activities, which are shown in Figure 1, are mainly related to the following: new products, marketing, research and development, production techniques, management procedures, quality standards, pricing. A firm may, for instance, say that 10% of its total turnover is due to its marketing efforts and 15% is due to activities which ensure good quality. Suppose now that the firm estimates that the influence of CERN has led to a 20% improvement in its marketing to other customers and helped to improve quality by 10%. We then conclude that $100 * 0.1 * 0.2 = 2\%$ of the total turnover is due to CERN's influence on marketing, and in addition, $100 * 0.15 * 0.1 = 1.5\%$ of the turnover is due to CERN's impact on quality. Generalizing this idea, managers can put percentage figures on the various activities of their firms and on CERN's influence on these activities.

purchases

It is, however, clear that often only some part of the total turnover of a firm is influenced by CERN (in Appendix B this is called "CERN Relevant Turnover"). From this relevant turnover,

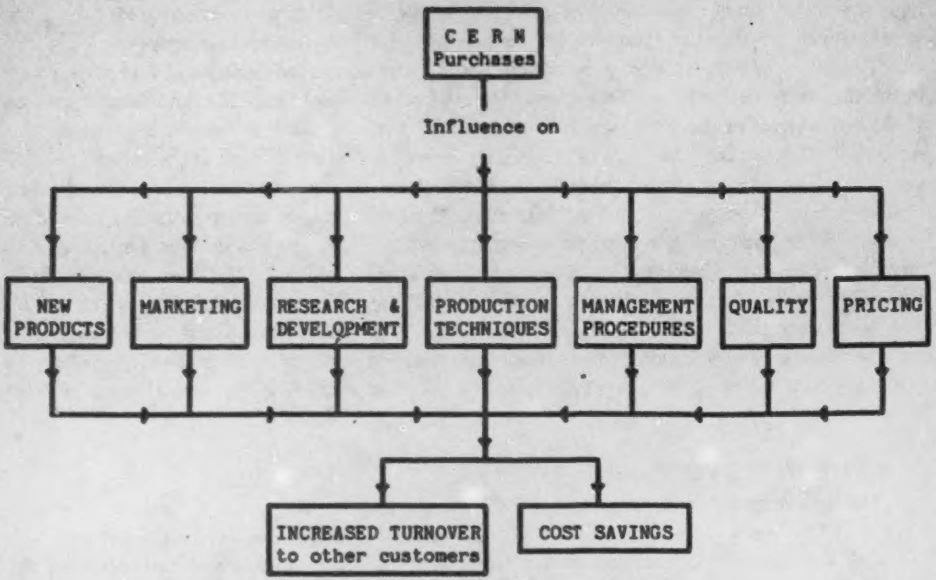

Figure 1: Principal secondary economic effects resulting from CERN.

the firm's direct sales to CERN must then be subtracted and the remainder multiplied by the above-mentioned percentages to give the increased turnover generated by CERN. Finally, any financial losses due to CERN, and any opportunity costs, or investment costs necessary to bring about the utility must be subtracted to obtain the increased turnover utility.

Possible cost savings are calculated in an analogous manner.

Since prices and influence factors are time-dependent, utilities are estimated by industrial managers in financial periods, each covering one year. As in previous studies, they relate not only to past events but also include, whenever possible, forecasts for the next five years.

III.3 THE SAMPLE OF FIRMS

CERN buys from approximately 6000 suppliers, about 10% of whom sell high technology goods. As it was considered necessary to make personal interviews in order to obtain accurate quantification data, and as the resources in time and money available for the present study were sufficient for about 170 firms to be interviewed, only a sample of the high technology suppliers of CERN could be considered in the present survey.

The first problem was to select the high technology firms from the total of 6000 CERN suppliers. Computer tapes giving details of CERN payments to firms for seven years during the period 1973 to 1981 (1974 and 1979 tapes were not available) were used for this purpose.

About 90% of the firms on the tapes are not high technology suppliers, and these were eliminated in two steps. Many were excluded by a decision to consider only those firms which had sales to CERN equal to or greater than 100000 Swiss francs in at least one of the seven years covered by the tapes. The resulting list contained more than 1200 firms, and checks revealed that only a small fraction of high technology suppliers was lost this way. By checking the firms remaining on the list individually, and eliminating those not supplying high technology equipment, a final number of 519 high technology firms remained. The results from the sample of firms interviewed have been extrapolated to obtain valid figures for the whole family of 519 high technology suppliers.

The family of high-technology firms was divided into two classes: firms which had participated in the first study (named "old" firms), and the rest (named "new" firms). It was decided to take about one quarter of the total sample from the "old" firms in order to be able to compare the results with those of the first study and to analyze the validity of the forecast utility values from the first study. This gives an over-representation of "old" firms; but, since a random sample was taken separately from both the "new" and the "old" firms, these two samples are representative each of its own group. In each group a stratified sample was taken, where the stratification (grouping) was made according to five industrial categories (see Table I) and two levels (less than, or greater than, one million Swiss francs) of the firms' total sales to CERN during the time-period being examined.

Apart from the grouping mentioned above, seven "old" firms were selected for the training of the interviewers. The selection of these seven firms was not done by random selection, and therefore the results for these firms were not used for the extrapolation to the whole group of "old" firms.

III.4 THE INTERVIEW PROCEDURE

The data were collected between May 1982 and June 1984. The group making the interviews consisted of the seven authors of this report coming from various disciplines. Each interviewer was provided with information concerning the firms, such as order volume and the nature of the material delivered to CERN. Before the interview, discussions were held with one or more CERN staff who had had technical contact with the firm. The most appropriate top-level manager in the firm was then contacted by telephone and asked if the firm would agree to participate in the study, and an appointment was fixed. In rare cases, where no appointment could be arranged, the interview was carried out by telephone.

To avoid misunderstandings, the interview was carried out, whenever possible, in the language spoken by the industrial manager. Interviews began with a short review of the first CERN Economic Study and stressing the confidentiality of any information provided by the manager, followed by a review of past and/or present contracts with CERN. Sales figures to CERN were checked, as well as the type of goods supplied, these figures being broken down, where necessary, by production branch of the firm. The type of utility created by the collaboration was determined using the quantification model. In some cases, where the utility was due to specific contracts obtained as a result of CERN influence, the manager provided absolute values of utility. Other information obtained, where possible, included distribution of surplus, origin of goods, comments on CERN's purchasing procedures, creation of employment, etc. Where a firm had participated in the first study, the previous forecasts for the first part of the period now being examined were compared with what really happened. It was helpful if the same manager could be seen on both occasions, as personal knowledge and memory play important roles in the estimation process, but, because of the effects of staff changes in ten years, it was in fact unusual to be able to do so.

During the interviews it was often possible to meet the director of the firm and/or the directors of various branches (technical and marketing directors), to visit the production lines, and to gain a better understanding of where problems had arisen, or could arise, when dealing with CERN. It proved necessary from time to time to explain the essentials of CERN's Financial Rules and related purchasing procedures, and to explain the way these rules have been laid down by the Council and subsequently checked for correct application.

A report, used exclusively by the members of the study group, was prepared after each visit made, and the information obtained was summarized in tables from which the data were entered into a computer for subsequent analysis.

CHAPTER IV

QUALITATIVE RESULTS

IV.1 GENERAL REMARKS

The industrial managers who were interviewed provided estimates of the increased turnover and cost savings due to the influence of CERN, but only occasionally gave information concerning the way in which these turnover increases have been achieved.

Increased turnover has in some cases led to the creation of employment, but many firms will probably have taken advantage of a certain amount of unused resources, and this has avoided the laying off of personnel.

The evaluation of the cost of increased turnover and the full quantification of the creation and maintenance of employment would require further investigations among the firms concerned.

Risk is a major obstacle to industrial innovation. When dealing with CERN the risk inherent in breaking new ground is likely to be less than when dealing with other customers. CERN can often help in solving technical problems, and failures are certainly not exploited by CERN to harm the industrial partner. After all, the only thing of interest to CERN is to obtain goods with the required quality within the time specified. All performance data on material delivered are available to the interested supplier, and this information is successfully used by some firms to save costs and/or to open new markets.

IV.2 IMPRESSIONS OF THE INTERVIEWS

Some interviews started cautiously, especially where CERN had not placed any orders for some years. On a number of occasions, although there appeared to be no probability of finding utility when the firm was first approached, positive results were nevertheless obtained during the interview.

In the branches where technological development was pushed by CERN, many firms appreciated the information and help given by CERN technical staff. This often helped them to overcome major problems which had not been foreseen in advance.

Marketing is an important factor, especially in the case of small firms which had not previously exported outside their own countries. CERN has succeeded in helping several such firms to initiate their export trade. The mere fact of having sold to CERN may be an important sales argument, although this was sometimes difficult to quantify.

CERN's quality requirements have helped some firms to penetrate overseas markets, or to be more competitive against overseas suppliers on European markets. In one or two cases, the quality requirements had a negative effect, because they were much above the level required by other customers. As expected, the CERN influence was acknowledged in connection with research and development and quality improvement. Quality problems were sometimes seen to have produced changes in management procedures and/or structures, although quantification of the resulting utility was not always possible.

During the interviews with firms which had incurred losses on CERN contracts it became clear that this often resulted from an under-estimation of the production costs. Some interviews also showed that firms may offer special conditions to CERN in order to have, or maintain, CERN on their reference list. The cost of this can be absorbed if the technology learned from CERN contracts is used to satisfy other customers or to introduce new products in the market; but difficulties may arise when the expected openings in the market do not occur.

IV.3 EXAMPLES OF CERN-GENERATED UTILITY

CERN-generated economic utility seems to grow on soil of all kinds, provided that at least one of several conditions is fulfilled.*) Some of these were found to be:

- the firm is active in advanced technology;
- a good professional relationship exists between CERN staff and staff in the firm with sufficient executive power;
- the marketing manager is aware of the fact that, for some potential customers, CERN is a good reference;
- the staff responsible for the firm's quality control have sufficient influence;
- the firm makes an effort to satisfy CERN's requirements, without necessarily making a profit while doing so.

The following examples illustrate these points:

i) TURNOVER INCREASE: Several European firms are fighting for a share of the high- and ultra-high-vacuum market. Their major sales argument is based on improved performance of the equipment offered to their customers. However, the development of a new or improved product is expensive and therefore risky. Under these circumstances, the large CERN projects with their requirements for a comparatively large number of units have reduced the risks considerably; and CERN has played a significant role in the firms' decisions to take the risk. CERN has significantly contributed to the breakthrough of several products on the market, and the firms in question were able to quantify CERN's contribution to the corresponding sales. The net result of this is that the European vacuum industry is well placed compared to the overseas competition.

ii) FIRM CREATED DUE TO CERN: A small firm specializing in precision mechanical components was set up about 10 years ago with the idea that the firm would be able to sell a reasonable percentage of its production to CERN. The firm was successful, and has regularly sold to CERN, whose influence on the success of the firm has remained at the same level as at the time the firm was created. At the beginning of the collaboration the owner of the firm had to develop special metal alloys to satisfy the CERN specifications. This has enabled the firm to remain competitive both on the home and export market, so that, when economic difficulties resulted in a drop in the home market, the firm was able to increase considerably its exports to other European countries.

iii) CERN AS A TESTBED: CERN may serve as a long-term testbed, since all performance data on material delivered are available to the interested supplier. This has been particularly true for suppliers of photomultipliers, and other special kinds of tubes; and especially in computing and related fields. Computer manufacturers test computers, computer systems such as networks, and software, extensively at CERN, where they benefit from the "know-how" and long-standing experience of CERN's experts. In particular, some firms test electronic equipment for reliability and performance at CERN before putting it on the market.

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*) Even if some or all of these conditions are satisfied, there is no guarantee that utility will be created, owing to the many existing obstacles to successful technology transfer or to unforeseen market developments.

iv) MAINTENANCE OF PRODUCTION: Among the problems of managing a shipyard is that of irregular production levels. One shipyard has on two occasions succesfully made bids for CERN contracts which enabled the yard to maintain its production level - and of course its work force - during slack times in its normal work of ship construction and repair.

v) INTER-COMPANY COLLABORATION: A small firm, which has supplied CERN with standard, but specialized, hydraulic equipment for about 15 years, entered into a commercial collaboration with several large firms because of contacts made at CERN. As a consequence of this collaboration the smaller firm's components are now standardized with the products of the larger firms, resulting in a large increase in turnover and exports to other countries.

vi) COMPLEX UTILITY WITH MANY INFLUENCE FACTORS: A firm was created more than ten years ago to produce precision mechanical products with a few employees. Three years after its creation it obtained its first CERN contract for mechanical components for electronic equipment which has since been widely used outside CERN. In order to keep abreast of competition and to be accepted as a CERN supplier over the years, the firm has constantly had to improve the precision and quality of its products. It did so by using the CERN specification as a sort of standard. It further successfully used the reference to CERN sales in order to acquire other customers, first in research, and later in other markets such as television, railways, subways, etc. A few years ago it undertook a major internal re-organization in order to increase productivity - a risk it was able to take because of the CERN orders and the resulting utility. Approximately half of this CERN-created surplus was re-invested, and the remainder used in equal parts to increase salaries, reduce prices and pay taxes. Furthermore, CERN has created or maintained, by its contracts or the related CERN-generated utility, a number of jobs within the total workforce.

vii) COST SAVINGS: A large electronics firm received an order from CERN for amplifiers which enabled the company to maintain its production levels when other orders were low. A certain amount of development work was required in order to satisfy the special needs of CERN, leading to cost savings which were used by the company to reduce sales prices.

viii) INNOVATION AND QUALITY: The innovations and/or quality improvements resulting from CERN contracts are often applied in fields which have no direct connection with high-energy physics. For example, the strict and complicated requirements of CERN concerning the optical and ageing qualities of light guides and scintillators has enabled one firm to develop products for solar energy applications. Since the technologies necessary to obtain the required optical properties of solar panels in production quantities are already known to the firm, it expects to be able to enter the market - which is only just beginning to expand - with a lead over its competitors. In this case it was still too early to estimate the full amount of CERN-generated utility.

ix) CERN AS REFERENCE: Being a CERN supplier can often be a useful reference for firms. In one case a firm making cooling equipment for CERN was able to gain admission to a trade association and, as a result, was able to obtain an increased number of contracts. In another case a firm building transformers used the fact that it was a CERN supplier to support its request for an international licence for certain products.

x) **NEGATIVE EFFECT:** CERN's quality requirements do not always have a positive effect on industry. In one case, the rigorous technical requirements of a CERN specification influenced a firm making electrical equipment in deciding to change its general assembly line procedures. The firm later discovered that these changes had resulted in a decrease in productivity.

CHAPTER V

QUANTITATIVE RESULTS

V.1 QUANTITATIVE RESULTS OF THE SAMPLE

It must be emphasised that (as was the case in the first study) the quantitative estimations of the CERN-generated utility were made by the industrial managers and not by the CERN interviewers. In case of doubt, the lowest figures were always taken.

Between May 1982 and June 1984, 166 European firms were interviewed, and 160 were able to provide useful information concerning utility. Of these 160 firms, 55 had no utility or could not quantify admitted utility, and 6 had made losses on CERN contracts which exceeded utility.

The period covered by the study runs from 1973 to 1982 for the firm's sales to CERN, and from 1973 to 1987 for utility created by CERN. Previous studies on utility showed it was possible to obtain utility forecasts for about five years into the future. However, this time, because of the present economic uncertainties, managers were often very reluctant to give such forecasts, which were nevertheless obtained, for at least one year ahead, from 78 firms. The five-year forecast period until 1987 was therefore maintained in order to give results which are comparable with the previous study, but it is clear that the utilities reported for the later part of this period are under-estimated (see Figure 2).

Between 1973 and 1982, CERN spent 748 million Swiss francs*) with the 160 firms of the sample which provided information, and these firms reported a utility of 3107 million Swiss francs for the period 1973-1987. The utility arising from increased turnover amounts to 2983 million Swiss francs and utility from cost savings is 124 million Swiss francs.

It should be remembered that direct sales to CERN have been deducted from the increased turnover utility, and that values are net, i.e. opportunity and other costs, and any losses due to CERN, have also been deducted. These data represent a lower limit of the utility actually generated by CERN, for the following reasons. First, it is the impression of all the interviewers that there is no systematic bias which would lead firms to inflate utility figures to please CERN or the interviewer. Second, it is possible that the manager interviewed may not have been aware of certain utility situations, or he may not have been in his present position long enough to have experience of the whole period under review. Third, there remain some utilities which managers were sometimes unable to quantify, particularly the reference value of being a CERN supplier.

Figure 2 shows the histogram of the interviewed firms' yearly sales to CERN and the resulting yearly created utility. The corresponding accumulated values are shown in Figure 3.

The SPS project lasted from 1971 to 1978 with payment of invoices extending into 1979. SPS sales appear to have produced their utility with a delay of a few years. The values shown for the period

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*) Financial data in Chapter 5 are expressed in constant 1982 prices (i.e. corrected for inflation).

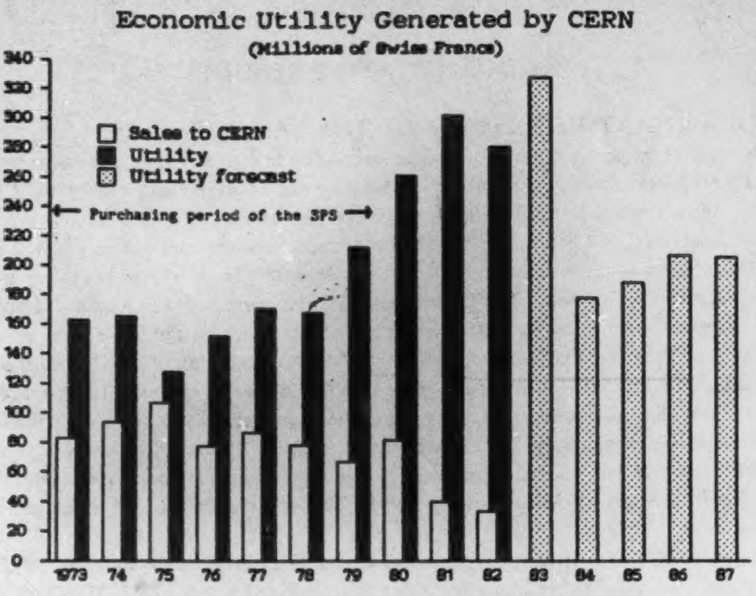

Figure 2: Yearly sales and yearly utilities from 160 firms interviewed.

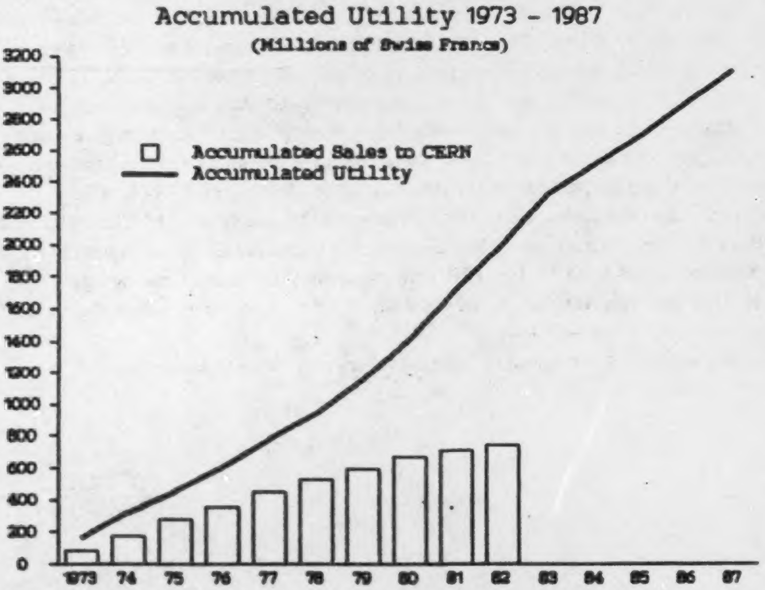

Figure 3: Accumulated sales and utilities from 160 firms interviewed.

1984-87 are comparatively low mainly because only about half of the firms interviewed made forecasts.

The utilities found for the various industrial categories are given in Table I.

Table I: Breakdown of Sample Data (160 firms) by Industrial Category

	Electronics, Optics, Computers	Electrical equipment	Vacuum, Cryogenics, Superconductivity	Steel and Welding	Precision mechanics	Totals
Net utility (MSF)	1576	877	355	225	74	3107
Corrected utility* (MSF)	1340	745	300	190	65	2640
Losses (MSF)	3.7	5.4	5.3	0.4	0.2	15.0
Sales investigated (MSF)	220.1	359.2	101.3	35.8	31.2	747.6
Corrected utility/sales ratio	6.1	2.1	3.0	5.3	2.1	3.5
Number of firms interviewed	57	46	22	16	19	160
Number of firms without utility	12	19	8	8	8	55

* Corrected utility equals 85% of net utility, for explanation see Section V.2.

Because of the complexity of modern European industry, it is very difficult, if not impossible, to determine exactly which country benefits from the utility generated. This is particularly true for multinational suppliers. Therefore it has not been possible to make any breakdown of the information by country.

A part of the increased turnover making up the utility consists of sales to the high energy and nuclear physics market. Although it was difficult to obtain reliable information on this for the full period of the study, reasonably accurate values obtained for 1982 showed that only 24% of CERN-generated increased turnover consisted of sales to the high energy and nuclear physics market. Indeed, CERN's purchases have stimulated technological improvements in fields which are not related to high energy and nuclear physics such as, solar energy, electrical industry, railways, computers and telecommunications.

V.2 EXTRAPOLATION OF THE SAMPLED DATA

The extrapolation of the utility obtained for the random sample to the total family of firms from which the sample wes taken was made using the group averages. A few of the firms interviewed reported utilities which were noticeably higher than the utilities of other firms in their group, and although these cases (outliers) could be explained, they were not considered to be representative. They were therefore excluded from the computation of group averages, and hence not used for the extraplolation.

Between 1973 and 1982, CERN spent 1379 million Swiss francs with the 519 high technology firms, and the total utility for the period 1973-1987 obtained by extrapolation amounts to 4796 million Swiss francs. Errors can occur in the extrapolation process from two sources: errors in estimating the group averages, and errors due to the difficulty of assigning multi-branch firms to the correct group. This extrapolation error (99% confidence interval) has been calculated to be ±623 million Swiss francs.

When comparing the 3107 million Swiss francs of utility for the 160 firms to the 1689 million Swiss francs of extrapolated utility for the remaining 359 firms, two points have to be taken into account. First, the sales to CERN by the firms interviewed already represents 57% of the total high technology sales to CERN. Second, the outliers contributed strongly to the utility of the interviewed firms but did not contribute to the extrapolated values.

Assuming that the utilities of the years 1973-75 are essentially the result of CERN orders prior to 1973, the utility corrected for this effect is about 85% of the net utility. The corrected utility/sales ratio then gives a measure of the utility created by CERN's high technology purchases made during the period 1973-82. For the sample this ratio is 3.5, which compares well with the ratio of 3.7 found last time.*) The overall (after extrapolation) corrected utility/sales ratio becomes 3.0.

Table 2: Breakdown of Total Data (519 firms) by Industrial Category

	Electronics, Optics, Computers	Electrical equipment	Vacuum, Cryogenics, Superconductivity	Steel and Welding	Precision mechanics	Totals
Net utility (MSF)	2638	1205	471	300	182	4796
Corrected utility* (MSF)	2245	1025	400	255	155	4080
Sales (MSF)	537.4	472.1	152.9	104.6	111.9	1378.9
Corrected utility/sales ratio	4.2	2.2	2.6	2.4	1.4	3.0
Number of firms	189	130	34	65	101	519

* Corrected utility equals 85% of net utility, for explanation see Section V.2.

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*) This ratio is based on data expressed in constant 1977 prices, which explains the difference to the ratio published in Refs.[3] and [4], which was based on data expressed in current prices.

Table II gives a breakdown, by category of firm, of the extrapolated utility generated by CERN in the total family of 519 high technology suppliers. Figures 4 and 5 show these data in graphical form.

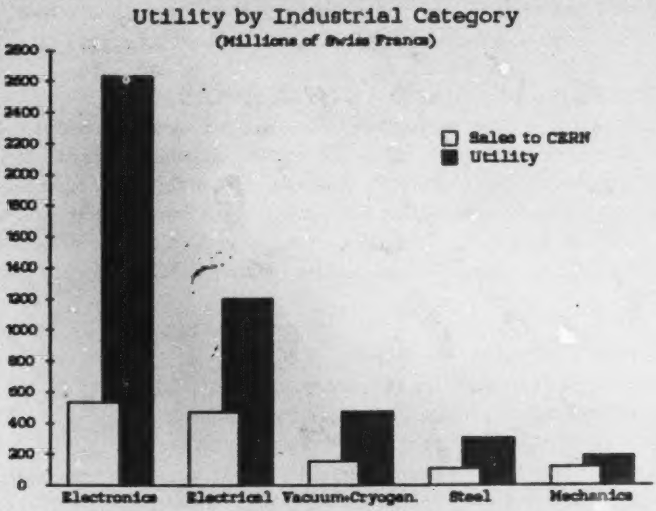

Figure 4: Total sales and total utilities from 519 high technology suppliers, broken down by industrial category.

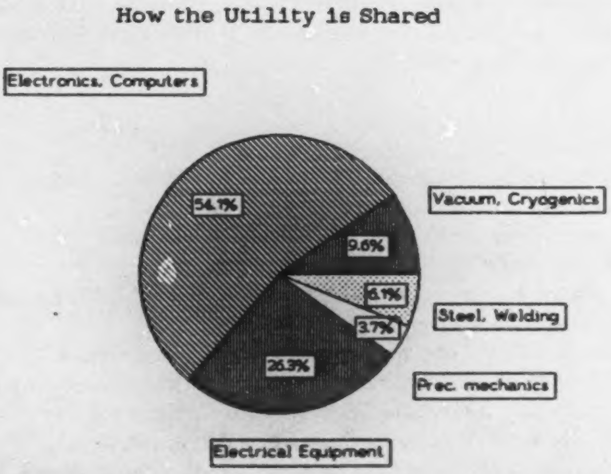

Figure 5: Total utility from 519 high technology suppliers, showing the contribution by industrial category.

The total cost of CERN in 1973-82 was 6945 million Swiss francs. The corrected utility/total cost ratio is therefore 0.85 * 4796/6945 = 0.59. Remembering that the firms interviewed have tended to provide low estimates of the probable actual utility, it may be stated that, by 1987, CERN purchases (high technology goods only) made in 1973-82 will have generated utility amounting to at least 59% of the total cost of CERN during the years 1973-82.

V.3 FORECAST VALUES AND ACTUAL EVOLUTION

During the first study most of the firms interviewed provided forecasts for the period 1974-78. About forty of these firms were selected by the random sampling process to be interviewed again this time, and they were questioned on the actual evolution of the utility during the forecast period. Out of these firms thirty-six provided data which could be compared to their previous forecasts.

This comparison showed that many firms actually realized utilities which were close to their forecasts, although in several cases the forecasts proved to have been too high or too low. This can be explained by:

- turnover of the firm different to the forecast,
- CERN effects coming earlier or later than expected, or not at all,
- losses due to CERN which could not have been foreseen,
- CERN influence on the firm different to the forecast,
- person interviewed unable to remember everything that happened ten years ago.

Out of the 36 firms, 21 had forecast utilities which turned out to be too high, and 15 forecasts which were too low. In spite of these variations, the total utility actually realized was within a few percent of the total utility forecast. In order to find out whether, on average, this difference between the forecast and the realized utilities was statistically significant, tests (t-test and signed rank test) for both the differences of the utilities and the differences of the utility/sales ratios were applied. None of these tests revealed a significant difference between the forecast and the actually realized utility values. We have therefore no reason to reject the hypothesis that managers can, on average, give correct utility forecasts.

Acknowledgements

We acknowledge the kind collaboration of the persons interviewed in the participating companies, without which this study could not have been achieved.

We wish to thank Professor H. Schopper, Director-General of CERN, Dr. R.F. Heyn, Director of Administration, and Dr. M. Lazanski, Leader of Finance Department, for their approval and financing of the project, and our respective Division/Department Leaders for agreeing to our participation and granting us the time necessary to carry out the study.

Even though we made use of computerized purchasing records, much other vital information was supplied by members of the Finance Department, and for this we are most grateful.

Our thanks go to the CERN scientific and technical staff who provided us with details of the orders placed with, and goods delivered by, the suppliers interviewed.

Finally, we have much appreciated and benefited from the help, comments, and suggestions of many of our colleagues throughout this study.

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APPENDIX A: THE THEORETICAL CONCEPT

The theoretical concept of the study described here has to fulfil three requirements:

- it has to be adapted to the level of information which industrial managers are willing to provide,
- managers must understand what information is required, and
- the concept must be such that the required information can be provided at all.

The "surplus" concept, as developed by Massé, Ref.[11], turns out to be an appropriate basis for this. The basic ideas of Massé's surplus concept are given below.

Consider an industrial firm which, for convenience, produces only one article. Let V be the number of units of the article it produces in year n. Let F_j be the number of units of the j-th production factor (manpower, machinery, materials, capital, cash resources, etc.) which the firm makes available to produce the V units of the article in year n. Suppose that all produced units are sold. If P is the unit price of the article and P_j is the unit price of factor F_j, the equilibrium equation (output = input) for year n is given by

$$PV = \sum_j P_j F_j . \tag{1}$$

Now, consider year n+1. Denote by $F_j + \Delta F_j$ the number of units of the production factors made available in year n+1 and use corresponding notations for the other quantities. For year n+1 the equilibrium equation must then be written as

$$(P+\Delta P)(V+\Delta V) = \sum_j (P_j + \Delta P_j)(F_j + \Delta F_j) . \tag{2}$$

We rewrite the equation (2) in the following way:

$$PV + P\Delta V + \Delta P(V+\Delta V) = \sum_j P_j F_j + \sum_j [P_j \Delta F_j + \Delta P_j (F_j + \Delta F_j)] . \tag{3}$$

Comparison of equation (1) with equation (3) shows that the first term on the left-hand side and the first term of the right-hand side of equation (3) cancel. Remembering that equation (1) describes the situation of the firm in year n and equation (3) in year n+1, we see that the terms left over in equation (3) describe the change in the factors, the prices and the produced units from one year to the next.

For proper economic interpretation, Massé rewrites equation (3) to obtain

$$P\Delta V - \sum_j P_j \Delta F_j = -\Delta P(V+\Delta V) + \sum_j \Delta P_j (F_j + \Delta F_j) . \tag{4}$$

The terms on the right-hand side of equation (4) represent a distribution of resources. The first, $-\Delta P(V+\Delta V)$, goes to the customers as the result of a price change (to their advantage if ΔP is negative), the second, $\Sigma \Delta P_j (F_j + \Delta F_j)$, is the allocation to the different production factors. The left-hand side must give, therefore, the gained resources to be distributed. Massé calls the left-hand side the firm's "gained surplus" and the right-hand side the "distributed surplus".

It is clear that a gained surplus can occur only if something in the firm changes. For instance, the firm may sell more due to better marketing, to better quality, to the introduction of a completely new product, etc. It may also have been able to increase productivity, to obtain better interest conditions on loans, or to improve its situation in other ways. On the other hand, if all the increased turnover is accounted for by increased production costs, both sides of equation (4) will become zero and there will be no gained surplus.

There must be driving forces which cause these effects to be positive. One of these driving forces might be a contact with a challenging customer such as CERN who asks a firm for new products, requires higher quality, provides a good marketing forum, and so on.

In a given year we may therefore consider a firm in two different situations.

a) The firm has no contact with CERN.
b) The firm has contact with CERN.

Using Massé's concept we can then define a surplus gained by the firm and a corresponding distributed surplus by comparing situation (a) with situation (b) in the same way as we did in equation (4) for the situation of the firm in two different years. If we observe a gained surplus we can say that this surplus was caused by CERN.

It may be very difficult, if not impossible, during the limited time of an interview for an industrial manager to provide information on CERN's influence on each different production factor and its unit price. He may, for example, be able to estimate the hypothetical development of only a few of the production factors. (Of course not all production factors are influenced by CERN. For instance, CERN has practically no influence on the capital cost (interest) of a firm). It was therefore decided to ask industrial managers to quantify only two elements contributing to the gained surplus, namely the sales increase and the cost savings due to CERN. In any case, these are the dominant elements and seem to be the ones which industrial managers can quantify with a reasonable effort. It is therefore necessary to replace the second term of the left-hand side of equation (4) by two terms, so that this becomes

$$P\Delta V - \sum_p P_p \Delta F_p - \sum_c P_c \Delta F_c = -\Delta P(V+\Delta V) + \sum_j \Delta P_j (F_j + \Delta F_j) \, . \qquad (5)$$

where, on the left-hand side, the second term represents the change in production costs necessary to achieve the increased turnover, and the third term represents the cost savings due to CERN.

Managers were asked to estimate the first and third terms, but were only occasionally able to provide information concerning the second. In order to express the fact that in this case we are not quantifying all parts of the gained surplus due to CERN, we call the sum of sales increases and cost savings due to CERN the utility U defined by

$$U = P\Delta V - \sum_c P_c \Delta F_c \, . \qquad (6)$$

In order to quantify the utility caused by CERN, a mathematical model was developed. This is described in Appendix B.

APPENDIX B: THE QUANTIFICATION FORMULA.

In order to provide a general formula which allows the quantification of the utility due to CERN, we denote the utility arising from increased turnover as U_s and the utility from cost savings as U_c. We then rewrite equation (6) as

$$U = P\Delta V - \sum_{c} P_c \Delta F_c = U_s + U_c . \tag{7}$$

In order to arrive at a simple quantification formula for U_s and U_c, we need a simple model of the mechanism of utility creation.

Taking a simplified view of the mechanisms which determine the success or failure of a firm, we assume that there are certain basic activities, such as launching new products, marketing, research and development, management procedures, manufacturing techniques, quality control, pricing, etc. The efforts devoted to these different activities, which we shall call "Economic Success Activities", bring about the economic results of the firm. Managers usually know the relative effect (not necessarily in money terms) which each activity has on turnover, production costs, etc.

He may say, for instance, that 15% of the firm's turnover M_t is due to their marketing activities. We then call the value 0.15 the marketing success factor for turnover. Such factors, by which we multiply the total turnover to obtain that part of the turnover caused by the corresponding success activities, we call "Economic Success Factors for Turnover". It is clear that the sum of all possible economic success factors for turnover must be 1, since all the success activities together cause the total turnover M_t.

In the same way, we define "Economic Success Factors for Cost Savings", which give the relative effects on cost savings M_c. Once again the sum of these factors must equal 1.

We denote the economic success factors by E_{as} and E_{ac}. The first index a indicates the success activity, while the second indicates sales s or cost savings c.

Using the economic success factors, we can quantify the parts of the total turnover M_t and the cost savings M_c which are due to the corresponding economic success activities. In order to quantify the utilities U_s and U_c due to CERN, managers have to quantify the influence that CERN has had on the different success activities of the firm. Here a manager has to compare the two different situations mentioned in Appendix A: (a) the firm in contact with CERN (real situation), and (b) the firm not in contact with CERN (hypothetical situation). If, for instance, a manager finds that CERN's contribution to launching new products was 30%, and if E_{ns} is the new product success factor for turnover, then the product $0.3 * E_{ns} * M_t$ gives the firm's turnover caused by CERN resulting from new products, and represents a sales utility due to CERN. We call the factor 0.3 the CERN influence factor on the new product success activity for turnover. Such factors, by which we multiply the corresponding economic success factor to obtain CERN's contribution to the corresponding success activity, will be called the "CERN Influence Factors on a Success Activity". Unlike the Economic Success Factors, the sum of which must equal 1, the CERN Influence Factors can take any value between 0 and 1. If all CERN influence factors are zero, CERN has no positive effect on the firm and there is no positive utility. We denote the CERN influence factors by C_{as} and C_{ac}, where the indices are the same as those used for the success factors.

The quantification equations for U_s and U_c therefore become:

$$U_s = (CE)_s * M_t , \tag{8}$$

$$U_c = (CE)_c * M_c , \tag{9}$$

with

$$(CE)_s = \sum_a C_{as} * E_{as} , \tag{10}$$

$$(CE)_c = \sum_a C_{ac} * E_{ac} , \tag{11}$$

where the index a on the right-hand side of equations (10) and (11) covers all economic success activities.

Some corrections have still to be made to equations (8) and (9) so that they apply to all cases. In equation (8) we have to take into account the fact that CERN usually influences only part of a firm. To illustrate this, consider a firm making transformers, electric motors and household appliances. Suppose that this firm has made electromagnets for CERN, and that this has subsequently led to improvements in the manufacture of their transformers and motors, but that there was no CERN influence on the household appliance business. The total turnover M_t of the firm in equation (8) must therefore be multiplied by a factor $C_t < 1$, which gives the fraction of total turnover in transformers and motors, influenced by CERN. We call $C_t * M_t$ the "CERN Relevant Turnover" and C_t the "CERN Relevant Factor" of the total turnover. Since the CERN relevant turnover still includes sales to CERN M_s , we have to subtract M_s from the CERN relevant turnover, since direct sales to CERN are not sales increases due to CERN's influence on the firm's economic success activities.

In addition, the resources used to produce the items sold to CERN could have been employed for other customers. We call these sales which would have been made elsewhere "Opportunity Cost" M_o , and this must also be subtracted from the sales utility given by equation (8).

Taking these points into consideration, the sales utility is given by

$$U_s = (CE)_s(C_t * M_t - M_s) - M_o . \tag{12}$$

Equation (9) must be expanded to take account of any investment costs M_i which were necessary in order to produce the cost savings, and we obtain

$$U_c = (CE)_c * M_c - M_i . \tag{13}$$

Combining equations (12) and (13) and taking into account any losses M_l which may have occurred due to CERN gives the final quantification equation

$$U = (CE)_s(C_t * M_t - M_s) - M_o + (CE)_c * M_c - M_i - M_l , \tag{14}$$

which, together with equations (10) and (11), is the basis for the quantification of the utility due to CERN. When applying equation (14), we have to take into account the fact that, in principle, all quantities are time-dependent. Therefore, the utility is calculated on a year-by-year basis, and is later expressed in fixed prices referring to a reference year, so that the utilities of different years can be added to give the total utility of a firm.

Dr. **Trivelpiece**. One of the comments that's made is that scientific research of this type certainly leads to advances, and if you look back 30 or 40 years you can see present value in things. But I think the time-constant is actually much shorter than that.

And in the abstract of this study it is pointed out that in Europe the CERN laboratory, over a period of about 10 years, had received funds from the European countries and had given contracts to European companies. These 160 companies over this ten year period booked some new business as a result of the contracts they had with CERN. Now, the difference is that 748 million Swiss francs was contracted by the European countries with European companies, and in return the companies did new business amounting to more than 3,000 million Swiss francs, in other words four times as much.

So over a brief period of 10 years, something like four times as much new business was booked by these companies. So it isn't just that building these kinds of accelerator facilities produces benefits to the nation for a long period of time; it produces a fairly short term benefit of significant magnitude.

And some of the products that those companies in Europe are now manufacturing are the very ones that we're concerned about in terms of our loss of competitiveness.

The other thing I would like to do briefly is call attention to two letters. We've received many letters in the Department of Energy about the SSC. One hundred and fourteen of them have been for and only two of them have been against the SSC. Frederick Seitz, who is the Past President of the National Academy of Science and President Emeritus of Rockefeller University, wrote a particularly thoughtful letter. I'd like the whole letter placed in the record, but I'd like to read just one paragraph if I may. He says:

> I reached the personal conclusion that our country should, indeed, take steps to provide the high energy particle community with still another machine, namely Superconducting Super Collider, generally known as the SSC. Such a machine would again place our own scientific community in a position of leadership in the study of the ultimate composition of matter and make certain that we have immediate access to whatever results emerge from the research.
>
> It must be recognized that as the European scientists regain something like the stature and capacity that they had before World War Two, they are more and more inclined to use their own equipment for their own purposes and allow American scientists to participate in a relatively peripheral way.

In a slightly different vein, the President of the University of California, David Gardner, says:

> Today our nation's future will in great measure hinge on our ability to capture and advance science and technology, the fundamental new knowledge and its applications that are the basis of economic prosperity and military security in what is for the United States a shrinking world. Despite its costs, the SSC represents an enormous opportunity for the United States, perhaps even an irreplaceable one.

So I'd like Dr. Gardner's letter, with your permission, to also be included in the record.

Senator **Ford**. It will be put in the record.

[The material referred to follows:]

September 4, 1986

IN STRICT CONFIDENCE

The Honorable John S. Herrington
Secretary of Energy
7B-058 Forrestal Building
1000 Independence Avenue, S.W.
Washington, D.C. 20585

Dear Mr. Secretary:

I very much regret that prior obligations in San Diego and Los Alamos on Monday, September 8, 1986, will prevent me from joining you for lunch and for a discussion of the Superconducting Super Collider. May I offer, by way of substitution, brief comments that I hope will be helpful.

From the time Ernest O. Lawrence built the first cyclotron at Berkeley in 1930, America was until recently the leader in the most fundamental of sciences, the study of the structure and nature of matter through high-energy physics. America relinquished that leadership role to Western Europe in the early 1980's. A research facility now under construction in Russia (UNK at Serpukhov) and facilities now being built (LEP) and planned (LHC) at Geneva in Western Europe, will be the premier world centers for high-energy physics for decades--unless the United States constructs the SSC. Thus, you will be offering advice to the President regarding the SSC that directly bears upon our nation's capacity to recapture our lost leadership in high-energy physics. As you consider what that advice will be, I would like to suggest three points you may wish to keep in mind.

First, it is quite unlike this country of ours knowingly to relinquish opportunities that are within our grasp, much less to stand aloof, when advantages we once possessed are in danger of being permanently forfeited to other nations. Yet if we do not build the SSC, the United States will lose irretrievably the momentum created by generations of scientists working in this country. It is entirely possible, in fact, that the United States may never be able to regain its former position of eminence in high-energy physics.

It is not irrelevant to note that significant numbers of our leading scientists, those who have spent their most productive years working in our country, were not born here. They came to America because it was here that they could pursue their scientific investigations most effectively, and it was here that other gifted scientists from throughout the world were gathering to advance the frontiers of modern science. Without the SSC, a new generation of young researchers worldwide will find other countries more attractive; it will be there, rather than in the United States, that the most exciting developments in high-energy physics will unfold.

Thus, it is not just a question of whether we construct a particle accelerator. It is also a question of whether the best and the brightest of the world's most promising high-energy physicists will pursue their investigations in the United States or somewhere else.

Second, the SSC promises critical breakthroughs in our understanding of the basic constituents of matter. While we cannot predict the intellectual, technological, and other gains that will come from what we learn as a result of the SSC, we know from history that basic new knowledge has led to revolutionary advances. Maxwell's synthesis of electricity and magnetism in the 1860's, for example, and his discovery of the laws governing electromagnetic waves, made possible our radio and communications industry of today. Quantum mechanics of the 1920's led to the semiconductor industry and the computer revolution.

Many fields will benefit from discoveries made possible by the SSC, as you know. Among them are accelerator physics, the physics and technology of superconducting magnets, data reduction and graphic display techniques in computer science, and ultrahigh-speed electronics, to mention just a few of the more familiar examples. And the SSC will generate improvements in areas already affected by the physics and technology of particle accelerators, among them synchrotron radiation as a tool in the science of materials; sensitive radiation detectors that reduce the amount of radiation needed in medical diagnostics; and computerized tomography, capitalizing on techniques for reconstruction of events seen in bubble chambers. I realize these and other examples are well-known to you, but I mention them simply as illustrations of the great potential of the SSC.

The new knowledge yielded by the SSC, combined with a vigorous program of technology transfer, will inevitably benefit a wide range of health care and high technology industries. As a result, the SSC will improve the quality of life at home and increase our ability to compete commercially abroad.

Third, the SSC should be seen not just in terms of its dollar cost but in terms of what the cost will be to us, as a nation, not to build it. The financial expense of the SSC is, of course, a major problem, even if the cost were reduced by international collaboration. Perhaps, in the present environment, it is the major problem. I cannot offer you much help here other than to call to your attention, and perhaps to the President's attention as well, that the United States did not grow and develop into an advanced society because the national character was either hesitant or timid. Over the course of our history bold and costly decisions were made that in due course moved this land to center stage in the affairs of nations. In some instances those decisions involved unpopular purchases of land, such as Jefferson's acquisition of the Louisiana Territory or Seward's purchase of Alaska ((described as a "folly" at the time). In other instances such decisions involved going counter to the popular current to prepare the nation for its encounter with a warring world in 1914 or with Fascism in 1938, when most people in this country were isolationist and insular in their thinking.

Today our nation's future will, in great measure, hinge on our ability to capture and advance the science and technology--the fundamental new knowledge and its applications--that are the basis of economic prosperity and military security in what is, for the United States, a shrinking world. Despite its cost, the SSC represents an enormous opportunity for the United ~~States~~, perhaps even an irreplaceable one.

Thus, you have no small decision to make this month. Please know how welcome would be the opportunity to help you in any way possible as you seek to decide this issue and, if your decision is favorable to the construction of the SSC, to assist in gaining the President's affirmative response.

With all good wishes, I am,

Sincerely,

/s/ David P. Gardner

David Pierpont Gardner

cc: ✓Director Alvin Trivelpiece

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE • NEW YORK, NEW YORK 10021-6399

October 29, 1986

The Honorable John S. Herrington
Secretary
U.S. Department of Energy
Forrestal Building, Room 7A-257
1000 Independence Avenue, S.W.
Washington, D.C. 20585

Dear Secretary Herrington:

As you may know, I served as chairman of the advisory committee of the National Academy of Sciences sponsored by the Department of Energy in connection with an in-depth study of the need for large scale equipment for materials research, an issue which has become of increasing concern to the research community - both pure and applied - as the frontier in materials research advances into regions where specialized synthesis becomes more important for technological advancement.

In connection with this study, it was inevitable that a small group of us who spent a great deal of time planning the program devoted some of our thoughts to another area namely that of high energy particle physics. Although this frontier lies almost entirely in the field of very basic concerns about the nature of matter, it is always difficult to predict what will come out of new research both as a result of new discoveries of a highly fundamental nature and the development of new equipment which may find extensive uses far from the original purpose.

I reached the personal conclusion that our country should, indeed, take steps to provide the high energy particle community with still another machine, namely the Superconducting Super Collider generally known as the SSC. Such a machine would again place our own scientific community in a position of leadership in the study of the ultimate composition of matter and make certain that we have immedate access to whatever results emerge from the research. It must be recognized that as the European scientists regain something like the stature and capacity that they had before World War II they are more and more inclined to use their own equipment for their own purposes and allow American scientists to participate in a relatively peripheral way.

Although the machine is judged in some quarters to be expensive, there is no reason why the cost cannot be phased over a substantial part of a decade so that they are not at all unreasonable when judged by our national expenditures for basic research. It is also true that the supporters of other fields of science sometimes believe that so much concentration of funds in one area may be to the detriment of others. We can all continue to trust that, as in the past, the products of good research will ultimately pay off many times the original investment.

It should be mentioned that we have at hand today in our country a highly enthusiastic and competent group of individuals who can undertake the task of designing and constructing such a machine. Moreover, there is ample evidence that several States are prepared to offer cooperation in providing part of the facilities. All of this momentum could be lost if we delay the program for an extended period of time.

Sincerely,

F. Seitz

Frederick Seitz
Past President
National Academy of Sciences

President Emeritus
The Rockefeller University

bc: Dr. Alvin Trivelpiece ✓
Dr. Edward A. Knapp

Dr. **Trivelpiece**. Up there with you, there should be a briefing book for the SSC. I would like to call your attention to about the second or third page, which has a little block around it. I would just like to close by calling your attention to this:

> Superconducting Super Collider is a new tool to explore the heart of matter. The SSC is essential to advance the state of knowledge in an area of basic research that is vital to our national interest. It is an outstanding investment in America's future that demonstrates the commitment to the pursuit of excellence in scientific research. Past investments in this field have been repaid hundreds of times over.

Parenthetically, I would like to note at this point that if you take into account our knowledge of the atom and its sub-atomic parts, the electron, the proton, and the neutron, and how that has then led to electronics and nuclear power and so on, you can make a rough estimate that something like a third of our gross national product probably owes its existence to our knowledge of those subatomic parts of the atom, and a good fraction of that is knowledge that has been gained in this area.

More importantly, and maybe the most important benefit of this, is that we'll renew some of our competitive abilities to pursue and inspire young people to pursue careers in basic research in advanced technologies.

I think the SSC, like other facilities, has that capability. It causes young people to visit them, to know about them, to take up careers in science. Whether or not they continue in this area of research directly remains to be seen. I think these serve as very important attraction coefficients, and we need these kinds of facilities in our country.

With that, Mr. Chairman, I would be pleased to answer any questions. Thank you.

[The prepared statement of Dr. Trivelpiece follows:]

Statement of Alvin W. Trivelpiece

Director

Office of Energy Research

Department of Energy

before the

Subcommittee on Energy Research and Development

of the

Senate Committee on Energy and Natural Resources

April 7, 1987

Mr. Chairman and Members of the Committee:

I am pleased to appear before the Committee to discuss the Superconducting Super Collider (SSC). President Reagan has approved proceeding with the SSC, the world's largest and most advanced particle accelerator. In announcing the President's decision to support this project, Secretary Herrington referred to the decision as one of tremendous significance and historic consequence, and a momentous leap forward for America and for science and technology.

The SSC is on the cutting edge of man's quest to understand the nature of the world in which he lives. Profound achievements along this path of research have included new insights and understanding related to gravity by Newton and Galileo, electricity and magnetism by Maxwell and Hertz, the atom and nucleus by Rutherford and Fermi, and relativity by Einstein. Scientists involved in high energy physics research today are at the forefront of this quest. This type of research has resulted in many U.S. scientists being recognized by Nobel Prize awards in recent years.

New concepts arising from high energy physics have profoundly altered the course of many sciences. High energy physics interacts with other areas of physics such as astrophysics, nuclear physics, atomic physics and condensed-matter physics. Achievements in accelerator research in high energy physics have enriched several fields of science. Some specific applications include: advanced electron microscopes, accelerators that allow us to see materials at the atomic level; synchrotron light sources and high-intensity neutron sources to study materials and biological molecules; and accelerator mass spectrometers to determine the age of samples from the earth's crust.

In addition, advances in basic knowledge contribute to economic and technological competitiveness through applications of the new knowledge and discoveries and through spinoffs. There are numerous instances of the impact of applications of technology flowing from high energy physics in the areas of nuclear medicine, electronics, computer chips, superconductivity, cryogenics, etc. Even though it is not possible to predict precisely when or where the application will occur, I am firmly convinced, based on the excellent track record of the field, that future applications will be as numerous and profound as those from past research. For example, approximately 100 million Americans each year receive diagnostic or therapeutic benefits from nuclear medicine which evolved from physics research. One example is the PETT scan (Positron Emission Transaxial Tomography), a novel non-invasive technique for directly studying the function of organs such as the brain. When the positron particle was discovered in the 1930's, many viewed it as an abstract curiosity of physicists. Today this abstract discovery provides the basis for an important tool of medical research and diagnosis. No one was able to identify this application when the positron was discovered 50 years ago. Today's quark is the positron of the 1930's.

Although scientists working in high energy physics have made great progress in understanding the ultimate building blocks of matter and the basic forces which govern the behavior and transformations of matter and energy, there still are many important unanswered questions. Continued progress in this research in the mid-1990's and beyond requires the study of particle collisions at energy levels that cannot be achieved with any accelerator now in operation or under construction. A new, higher energy accelerator is essential; the SSC is a facility which can penetrate the higher energy domain and address the unanswered questions. It will be the world's preeminent facility for high energy physics research.

The SSC will be the world's most advanced particle accelerator facility; its energy level of 20 trillion electron volts per beam will be 20 times the present highest energy available in the world at the Tevatron near Chicago, Illinois. As currently envisioned, the SSC will be a 20 TeV on 20 TeV proton-proton collider, the heart of which is two rings containing about 10,000 superconducting magnets located in a racetrack-shaped tunnel about 53 miles in circumference and 10 feet in diameter. These magnets will focus and guide two beams of protons in opposite directions around the tunnel. The collisions that result when the two particle beams are boosted to nearly the speed of light and allowed to collide head on will create new particles that will be detected and analyzed. There are four interaction regions at which experiments will be carried out with detectors that record the results of the collisions between protons. In order to do physics research, particle detectors for each of the interaction regions and a large central computer will be required. The project also provides laboratory space and technical support facilities.

The SSC will provide a valuable training ground for American students, scientists and engineers. Groups from over 100 U.S. universities are expected to use this facility. It will be an attraction for many of the world's leading scientists to live and work in the U.S. The SSC has the kind of dramatic appeal that inspires young students to pursue careers in science and technology. It will provide a unique challenge to a new generation of the best and brightest young people, the challenge to unlock the secrets of nature.

R&D studies for the SSC were formally initiated late in 1983 in conjunction with special Congressional hearings. The R&D was conducted at leading high energy physics laboratories and other institutions as a national effort under the coordination of the SSC Central Design Group (CDG) formed by the Universities Research Associates, Inc. A detailed Reference Designs Study (RDS) was completed in March 1984. This report, which was carefully reviewed by the Department, established the basic feasibility of the SSC, provided a preliminary cost estimate, and identified R&D needs. Following this, much R&D was done to verify the assumptions of the RDS and to provide the basis for selection of the magnet type for the SSC. Major R&D achievements were made, particularly in the areas of superconducting wire and magnet heat loss. The RDS was based on the assumption that SSC superconducting wire would have a current carrying capability 33 percent better than the Tevatron's. A 50 percent improvement has been demonstrated in industrially produced wire, with even higher performance achieved in laboratory tests. The heat loss for SSC magnets was projected to be only 10 percent as large as that of the Tevatron magnets. This major improvement in performance has been demonstrated. In 1986 the CDG completed the SSC Conceptual Design Report which was thoroughly reviewed by the Department of Energy with the aid of many independent experts. This process led to the conclusion that the project was technically feasible and that the cost and schedule estimates were sound. The project also has been reviewed by the High Energy Physics Advisory Panel and has earned the strong support of the high energy physics community.

As the FY 1988 budget process evolved, the Department of Energy also thoroughly reviewed all aspects of the project via its established major systems acquisition review system. The Secretary publicly announced his support of the project in December 1986 and indicated his intention to seek Administration approval for the SSC. The Administration's deliberations concluded with a presentation before the President and the Domestic Policy Council on January 29, 1987. The following morning, President Reagan gave his approval to proceed with the super collider, and Secretary Herrington announced the decision that afternoon.

The total project costs of the SSC are estimated to be $4.4 billion in FY 1988 dollars, including the costs of construction, R&D, detectors, computers, and preoperations. In escalated dollars, this becomes $5.3 billion. We estimate that with an FY 1988 project start the target completion date of SSC is 1996. In FY 1988 we are requesting $10 million for construction-related activities and $25 million for R&D related to SSC.

No determination has been made as to where the SSC will be sited. However, the Department will follow an open, competitive and equitable site selection process. A special SSC Site Selection Task Force has been formed to develop and implement this process. The process began with the Department's issuance of an Invitation for Site Proposals last week. Proposals are due to DOE in August 1987, and the preferred site is to be announced in July 1988. We anticipate that the site will be provided at no cost to the government.

We are actively seeking maximum cost-sharing from foreign countries, industry, and state and local governments. Such cost sharing could take various forms, including in-kind contributions such as magnets, detectors and other components, as well as direct financial involvement in design, construction and/or operations. As you know, there is a long and successful history of collaborative research efforts in high energy physics. The value of cooperation in research and facility construction can be measured not only in the research results that flow from the cross-fertilization of ideas, but also in the savings that result from avoiding the duplication of costly facilities and equipment. The value of international cooperation was recognized formally by President Reagan and other leaders of the Economic Summit Nations in 1982 and has been reaffirmed in the ensuing years. International interest in the possibility of contributing to the super collider has been expressed in follow-on discussions with the six other Economic Summit Nations (Canada, France, Germany, Italy, Japan, the United Kingdom) and the European Communities. Although I obviously can indicate no definite commitments at this time, I am confident that working closely with the Department of State we can achieve significant international participation.

The Department is proud to present this request to the Congress, and we look forward with great anticipation to this historic undertaking.

Mr. Chairman, that concludes my prepared statement. I would be pleased to answer your questions at this time.

Senator **Ford**. Thank you, Doctor.

You have covered a large field very quickly. Just give us your view of what the most important reasons are why we should build the SSC? You have laid it all out, but if we had to put it in capsule to go out there and sell it, what are the reasons we should go forward with it?

Dr. **Trivelpiece**. Certainly the ones I just cited: inspiring our youth to pursue careers in science; that the past experience in research in accelerators has shown that this is an incredibly good investment in science and technology, it's been repaid hundreds of times over.

The kind of facilities like synchrotron light sources, which are themselves accelerators, these accelerators are now being used to make X-ray light sources, and those X-rays are being used to make the next generation of supercomputer chips, which will be in the next generation of supercomputers.

It's hard to limit it to one or two, but the examples keep going on. The positrons, which were a very exotic elementary particle in the thirties, they are now at the heart of the Positron Emission Transaxial Tomography or the PET scans which are being used to understand the difficulties of Alzheimer's syndrome and other neurological disorders.

You ask a difficult question, how to think of a single item that would sell somebody or cause them to agree that this is the most important thing to do. I can think of hundreds. I have trouble thinking of two.

Senator **Ford**. Well, that is like, if you want a five minute speech give me an hour to think about it; if you want an hour speech, give me five minutes to think about it.

This is one of the important questions we are going to have to answer—and I am not sure many of us will be able to give the detail that we should be able to give. But give us some sort of feel on the return on investments that our taxpayers would receive from the expenditures on this major project?

Dr. **Trivelpiece**. I'm not sure what you mean? The federal taxpayers or the state taxpayers?

Senator **Ford**. Well, where there is going to be some money invested, and it is going to be basically tax money if we are talking about $4 billion or $5 billion, and it basically comes from the taxpayers.

What can they expect as a return from their investment?

Dr. **Trivelpiece**. Again, you're asking a question that is difficult to answer in the predictive sense. It's easy to answer in the retrospective sense. As I said earlier, something like a third of our GNP probably owes its existence to our knowledge of the atom and its sub-atomic parts.

If you think of a third of our GNP and ask, what is that returning to the Treasury today, it's probably returning substantially more than is spent in all the basic research supported by the Federal Government across its broad base. So how do you decide what particular activity area is going to result in the greatest payback?

People who were curious about the decay of light in certain solids back in the late fifties were probably interested in inventing lasers, but nobody had invented one. That was the basis on which lasers were invented. What is the return on that investment today?

It's easier to go back and find activities and then talk about the present value. I simply don't know how to make a prediction about the forward value of this, other than it will continue to contribute to the base of knowledge that has been of great importance to our nation.

Senator FORD. Now, can we just take everything you just said as a positive and say, that is what we will lose if we do not go forward with it?

Dr. TRIVELPIECE. That's my view.

Senator FORD. You are advocating a large federal construction project that would substantially increase the budget over our current services. And your colleagues in the Administration are denouncing, as I said earlier, Congress for excessive spending.

What options did you consider to find the funds for the SSC by some other means, other than increasing the deficit that would otherwise occur?

Dr. TRIVELPIECE. The issue didn't come up in the way that you're raising the question, but the issue did come up, certainly. The way it came up was that Secretary Herrington was concerned that this was something that was not going to be done within the ordinary budgetary constraints of the Department of Energy. To have this be a policy issue he took it, rather than through the budget process, to the Domestic Policy Council, to ensure that it would be clearly understood by the Administration that, if we are to proceed with this, we are going to proceed with it in terms of "new money."

Now, what does "new money" mean in this context? It clearly means some adjustment, either to the deficit or to the revenues, or being spread elsewhere across other programs in the federal government. Obviously that decision on a "put and take" basis hasn't been made, and even if it is made I suspect tracking it in an audit-trail sense is probably an impossible thing to do. What you need to be assured of, I guess, is that next year when the request for the first large construction increment comes, is that the Administration is not doing it in a way that it takes from the other basic research programs with which it competes.

I think this was one thing we were very clear on, that the SSC should not proceed if it is to come at the expense of basic research, and, in fact, Secretary Herrington supported the increase of the National Science Foundation's budget, which amounts to a doubling over the next 5 years, which over a 10-year period amounts to about $10 billion of new money.

That, taken together with the SSC at $4.4 billion over the same period, is really an unprecedented request for an increase for basic research. I think it's because it is recognized the important role that such an activity plays in our national fabric is recognized.

Exactly where that money is coming from, I don't know that anyone has addressed it in the sense that you're asking, of a particular auditable "put and take."

Senator FORD. Do you not have a gut feeling, if I can use that term, that it is inevitable that we will have to have some sort of tradeoff, if we go to this major project, with other science programs?

Dr. **Trivelpiece**. Well, I hope not. I mean, it is not the Administration's position that that is what we want to see happen. It is our position that these other science programs should be done.

Where the tradeoffs are made is difficult to say, and obviously that debate takes place in the Congress. We didn't make a specific proposal. But only in the other sense, that this should not come at the expense of other basic research activities within the Department or elsewhere.

Senator **Ford**. Doctor, and I am not fussing at you, but it is typical that here comes a $5 billion request in addition to the present budget, without any backup, any formula for raising that kind of money except cut other scientific programs.

This Committee, I think, has been as concerned and as considerate as they can be to continue some of our research programs, even maybe at a lower figure than we would like. But we have already made tradeoffs to secure and absorb the $35 million of the high energy physics budget for 1988.

And how are we going to allocate any additional cuts? It bothers me considerably that we are being proposed here a major, major expenditure, without any way to offset that expenditure other than cut others, and yet it is left to us to cut.

We go on the Senate floor now—and Senator Domenici is here—and after we have a budget resolution, if we propose additional expenditures we have to have in that same piece, on that same piece of paper, the method whereby we are going to find the additional money, either take it from one program and give it to another or propose an additional income.

So I am very concerned that we are going to lose something that is very important without the Administration's push as it relates to finding the additional funding.

Dr. **Trivelpiece**. This is not a problem in 1988. It becomes a problem in 1989.

Senator **Ford**. I understand that. But we are already doing 1988's budget and less than a year from now we will be doing 1989's budget.

Dr. **Trivelpiece**. It is incumbent on the Department to make proposals so that this is a request for additional funds. I think that was why Secretary Herrington took it directly to the Domestic Policy Council and the President, to ensure that there was an understanding that this was not going to be done within the already existing budget envelope which the Department has.

Now, that needs to be left up to next year. I have no idea how it will come out, but I am hopeful that it will come out that it will be a request for additional funds. Where that gets spread elsewhere in the Administration's budget request remains to be seen.

Senator **Ford**. Well, it remains to be seen.

Senator Johnston.

The **Chairman**. Thank you, Mr. Chairman.

Dr. Trivelpiece, as I mentioned earlier, I am a supporter of the SSC. But there is another side and the other side, at least part of it, was presented rather eloquently in writing by Philip W. Anderson, who as you know is a Nobel laureate, who submitted a written statement, who cannot be here. And let me submit a couple or two or three of the points he made and ask for your reply to them.

[The prepared statement of Dr. Anderson follows:]

TESTIMONY ON THE SUPERCONDUCTING SUPERCOLLIDER

(By Philip W. Anderson)

I am sure the case for the SSC will me made ably by other witnesses. I would like to lay out the scientific case against it, because I think many of those colleagues who understand this case are hesitant to make it, not least because some of the arguments are two-edged. I am certainly very hesitant in doing so myself, because I am not against the project, except as a competition for the resources which I see as more needed elsewhere.

Let me organize my testimony in terms of four slogans, each of which is aimed at sowing doubt about one of the myths supporting the unique value of elementary particle physics. To support each slogan I will then try to discuss concrete examples.

(1) Science can be fundamental without being irrelevant.

(2) Money is important but manpower and education are more so, and money affects these.

(3) "Spinoff" should be erased from the language.

(4) The golden eggs are very seldom produced by the golden geese.

1. The first slide in many general talks given by my high-energy colleagues is a length scale spreading from the "Planck length" (way below elementary particle size) to the size of the cosmos. They gesture deprecatingly towards the center of this scale (where we and our atoms and all of everyday life sit) and say, "of course, we know everything there, and the only fundamental science is at extreme scales." Well, we don't "know everything there": we haven't the foggiest idea what drives the new high-temperature superconductors, or what makes a snowflake, or how the mind or the economy works. What is more, nothing high energy physics can do will ever be of the slightest direct help in solving these overwhelmingly hard problems.

The reason is the idea of "emergent properties:" that complicated systems have properties which are not implied by the elementary pieces of which they are made. To a brain or a computer, it just doesn't matter what the physics of the hardware is, for example. We have long since learned everything particle physics can tell us about the behavior of ordinary matter, even of nuclei, and probably of the stars themselves. If the particle physicists tell you they will understand even the Big Bang better as a consequence of the SSC, they are being wildly optimistic; and if they claim any other relevance, they are wrong. Their fundamental physics has become so "fundamental" as to be almost totally irrelevant, even to the rest of science.

On the other hand, really fundamental ideas about how complicated systems work can arise in very unlikely places. Recently, there has come into being a whole new form of statistical mechanics (the science of the thermal behavior of matter) contradicting some of the oldest beliefs in the field. This new statistics has suggested revolutionary ideas in fields as wide apart and as important as neural modeling of memory and other thought processes, the dynamics of protein, computation and complexity theory, and evolution. The field of solid state physics from which it arose is called "spin glass' and from the paltry million or so dollars of funding it once had, it has been reduced by the NSF to zero: investigators are warned not to use those words in their proposals. Yet I think this low-budget, unfashionable field still has much fundamental to tell us, as do literally tens of other I could name — as for instance metal-insulator transitions and oxide superconductors, which four months ago were equally unfashionable.

2. I know that to Congress money is important, and it is the appropriate way to set priorities, but the particle physicists have a way of comparing the cost of the SSC to that of a battleship or a missile and making it out as not very big on that scale.

The catch there is that that much money spent on this kind of science is going to have a large effect on how we use our really bright, well-trained technical people, and on how we educate them. They are a resource which is extraordinarily scarce and getting much scarcer: so scarce that it is beginning to be a surprise to hear an American accent in the physics departments of this country, because we are having to fill in more and more with immigrants and visitors in our junior positions.

Particle physics does not dominate physics employment in this country, though it could grow a lot with the SSC; but more important, it has an enormous influence on physics education. It (and other big-ticket physics) dominates many of the most prestigious institutions because the spare change from large contracts in particle physics or astrophysics can be used to support junior teaching positions, the glamor of which attracts very able young people in spite of the insecurity of their tenure. It also dominates the faculties of many of the smaller departments because there is no employment for the expensively trained particle or astrophysicist outside academia, unless he changes fields, while other physicists — atomic, solid state, etc. — can easily get jobs in industry or government laboratories. I hear again and again from students that they just never heard anything from their teachers about any other branches of physics until they reached graduate school, if then.

Our colleagues, even the particle physicists, in physics departments are neither wicked nor dumb: they would very much like to hire in their departments able colleagues who do bench-top physics in-house and can train the many students who not want to do particle physics. But literally dozens of universities around the country are looking for one or more such people and not finding them, since they are very scarce: among other reasons are that universities don't compete with industrial salaries (why should they when particle physicists are always available) and, even more important, they cannot protect the young physicist from the hassle for the perpetually dwindling budget for small-scale fundamental science in the universities.

There is another non-negligible effect. The popular literature about physics is dominated by particle physics and astrophysics to the near exclusion of any other part of physics. I recently was asked by a science writer to recommend a good popular book on solid state physics, and I had to admit that the only one I knew was 30 years old. This situation is exacerbated by the fact that the simplicities of particle physics are easier to explain than the complexities of condensed matter, and by the glamor and glitz surrounding Los Alamos, the Bomb and the big machines, but it also partly has to do with the number of unemployed or underemployed particle physicists who become science writers.

I am perhaps less worried by the nature of the elementary particle experience for the student than some of my colleagues: for the experimentalists, collaborative experiments so large that the personnel fills convention halls, and the experiments take three graduate student lifetimes, and for the theorists, a total loss of touch with the real experimental world. But perhaps the only true spinoff from particle physics is its discards. For instance, without hundreds of them, literally, the electronic switching network at AT&T would not have been built, since we at Bell Labs found them very useful after some retraining. The nature of the work in large technical projects is quite similar no matter what they are aimed at. But is that the way we really want to spend our scientific budget and do our education?

3. One of the great arguments for spending money on large technological projects is always "spinoff" — I need hardly remind you of some of the military projects justified (speciously) by that word. Where I spent much of my working life, at the AT&T Bell Labs, however, "spinoff" was a dirty word, since we knew how technological innovation really happens. One example of "spinoff" which is adduced for large accelerators is superconducting magnets, and I am very familiar with that story, since I was closely associated with the group at Bell labs which invented them in 1960, not as a spinoff from anything but as a consequence of a fundamental research program on superconducting materials, with no public funding and probably few prospects of getting any even then. (I probably am more responsible than anyone for explaining why superconducting magnets work.) Their further development was indeed carried out partly by wisely administered public money from the Service Agencies and partly by industry, and then by small instrumentation firms concentrating on scientific uses. Federal funds gradually dried up, until in recent years, again, maintaining a program on superconducting materials was a recipe for funding disaster.

The stimulus for large-scale industrial development in the end came mostly from chemical and medical uses, in the early days of which again the Bell Labs played a vital role through its fundamental biophysics research. As far as I know the truly innovative part of this technology owes nothing to particle physics.

There is a reason for this. Any good engineer will tell you that the last thing in the world you want for your large engineering project to depend upon is truly innovative technology. Innovation in one piece of a project is permissible — you may only lose your deadline. But more than one truly innovative piece spells disaster. The delays and mistakes inevitable in new technology are just too common. This is a lesson that AT&T long since learned, and I could wish the Pentagon knew it better; but it means that spinoff of really novel technology is the last thing the manager of a large, difficult project like the SSC really wants, if he wants to hold his budget. Look at Isabelle!

4. Those who manage the funding of science, including Congress, I am afraid, have a very strong prejudice in favor of large expensive projects and large unitary laboratories or "centers." In the case of politicians the motivation may be the relatively innocent one of the pork barrel, or the sometimes less innocent one of being identified with — and sometimes controlling — a tangible institution. In the case of the agencies, the motives or at least the results almost always involve some degree of management and control, often but not always obtrusive micromanagement. Be that as it may, the amount of money available for free, unprogrammed individual research dwindles and dwindles. I note that once again this year small independent fundamental research in physics and materials science is to be cut back by the NSF, and more money yet again funneled into large facilities and into so-called "centers", even if the piece of NSF money earmarked for SSC is not used.

Unfortunately, it is not the large expensive programs that produce really new things. Again I'd like to take my examples from superconductivity becasue I know it so well. In the first place, of the last three major developments in superconducting materials none of the breakthrough discoveries took place in the U.S., the Chevrel materials being discovered in France and Switzerland, heavy electron superconductors in Germany, and high T_C oxides in Switzerland. The program in Switzerland which led to the recent spectacular achievements would probably not have been funded in this country. Even once La_2CuO_4 appeared the next confirmations appeared in Japan and then in Beijing. The materials themselves were primarily studied heretofore in India and in France.

Part of the reason for this preponderance of work overseas has to do with poor funding of small science here, and part of it with a channeling of what research we do into large facilities. A large facility is not something on which you make breakthrough discoveries, and on which serendipity can operate, mostly. Instead it is something on which you test breakthroughs once you have them. But in any case our level of function on large facilities is not too high; it is that the innovative, small group work must not be cut back in their favor.

In conclusion, let me say that I too want to know what the ultimate structure of matter in the world is going to be, as well as how the Big Bang happened, and I want to see an accelerator going far beyond the present limits eventually built. I do not accept that doing so is as urgent nationally as many other scientific needs of the country, in space science, science education and above all in the rescue of our strong tradition of innovative, fundamental small science, which is now being carried on in only a few universities (such as Cornell), industries (such as Bell Labs) and government institutions (such as Los Alamos), all in a somewhat embattled state. A fraction of the budget earmarked for SSC could easily restore our prominence in this area, if given — say — to NSF and restricted to small science uses.

It disturbs me to see accelerator physics seen as a nationalistic, competitive race; science is too serious a matter for that. And if the lack of the right accelerator here at exactly the right time is really going to kill high energy physics, I must say it is better off dead, if only for the crippling lack of imagination that that reveals.

POSTSCRIPT

I cannot conclude without making a brief remark about the incredible new developments in superconductivity. A few weeks ago (when the above was written) I thought that the new superconductors would develop too slowly to influence the SSC. This is not true. The pace of actual technical development revealed at the session of the APS meeting in New York on March 18–19 is unprecedented, and it is possible to anticipate that one can either omit Helium cooling or reduce the size of the ring or both within a matter of 2–3 years. It would be very unwise to fix the design of the SSC now.

NOTES ON PWA:

I am primarily a solid state (as we now say, condensed matter) theoretical physicist, although my thesis is still often used in atomic and molecular physics, I am given credit for first describing a key part of the modern synthesis in elementary particle physics, and I have contributed seriously to pulsar physics. I received the Nobel Prize in 1977 and the National Medal of Science in 1982 among other honors, and was a council member 1976–9 of the NAS. I am Joseph Henry Professor of Physics at Princeton.

The CHAIRMAN. First of all, he says, in a postscript that he thought the new superconductors would develop too slowly to influence the SSC when he first thought about it, but: "This is not true. The pace of actual technical development revealed at the session of the APS meeting in New York on March 18 and 19 is unprecedented, and it is possible to anticipate that one can either omit helium cooling or reduce the size of the ring or both within a matter of 2 or 3 years. It would be very unwise to fix the design of the SSC now."

What is your reply to that?

Dr. TRIVELPIECE. I strenuously disagree with his position, and I've talked to quite a few people about this. Probably in terms of an ability to respond to these recent superconducting discoveries, the Department of Energy has done about as well as anyone has, in an almost immediate redirection of activity.

Something like $15 million, on an annual basis, in effort is going into this. It's an absolutely important and astounding discovery. It should be taken great advantage of, but I would remind you of the process that one has to go through in making something like a magnet that's used in accelerators such as the accelerator at Fermi Lab now.

First you have to make the material, then you have to be able to fabricate it into wires, the wires have to be fabricated into cables, the cables have to be made suitably reliable, ductile, capable of being fabricated into magnet systems. The magnet systems have to be designed in such a way that the heat loss is acceptable. They have to be capable of being operated in groups of 10 or 15 or 20. There's a considerable amount of engineering work in terms of reliability. You don't want to have a system of 1,000 or 10,000 magnets in which the mean time to failure for a magnet is a matter of a few days.

There's a tremendous amount of engineering work that goes beyond the point of where the superconducting discovery is right now, and remember, this is a material which is ceramic. This is not a ductile material to begin with, so the first thing you have to do is to take a glass-like material, make it, fabricate it into a ductile conductor, and then go through all these other steps that I was talking about.

Pierre Grande from France, with whom I talked about this the other day, says he doubts that this is possible within 15 years. And I think that's an estimate that many of us would feel comfortable with.

Plus the process for the SSC, if it were to start on the schedule that we're proposing, would need to begin buying materials for it with some of these funds that we've requested in the Fiscal Year 1988 budget. It's hard to imagine that that development could proceed at a pace that would make it possible to do as Dr. Anderson suggests.

I'm quite dubious about that possibility, even though there are some things that might happen in the superconducting business that would be very important. Others have suggested that these materials are suitable for radio frequency superconducting cavities; that's a more doable kind of thing. And then the cavities that are used in accelerators might be capable of being built with these new materials.

There is no development work yet, however, to prove that that's true either.

The **Chairman**. Like SDI, this is way out in the future somewhere?

Dr. **Trivelpiece**. Well, it's certainly beyond the time period in which it's likely for us to have to make the decision in terms of proceeding with the SSC if we're going to do so on the schedule we've got planned.

The **Chairman**. Point number two:

> We have long since learned everything particle physics can tell us about the behavior of ordinary matter, even of nuclei, and probably the stars themselves. If particle physicists tell you they will understand even the big bang better as a consequence of the SSC, they are being wildly optimistic. And if they claim any other relevance, they are wrong.
>
> Their fundamental physics has become so "fundamental" as to be almost totally irrelevant, even to the rest of science.

Dr. **Trivelpiece**. Well, you're now pointing to an activity which I think properly belongs in the American Physical Society meetings. This is a matter that should be debated among peers.

If Dr. Anderson has that view, he should make it known at the next APS meeting and provide an opportunity for people who are currently working in high energy physics to have equal time and debate the matter with him. I'm not sure how that discussion would come out, but that's the proper arena for it.

The **Chairman**. I mean, we've got both sides represented here, him in writing and you in person.

Dr. **Trivelpiece**. I don't agree with him personally, but then, I'm not the one who should be debating him on some of the merits of the high energy physics research, which is not my particular field of research. There are others here—Steven Weinberg and others who are going to testify later—to whom I think you really should address that question.

The **Chairman**. I will let those others later testify.

The third argument:

> One of the great arguments for spending money on large technological projects is always "spinoff." I need hardly remind you of some of the military projects justified, speciously, by that word. Where I spent much of my working life, at the AT&T Bell Labs, however, "spinoff" was a dirty word since we knew how technical innovation really happens.

He goes on to say that:

> Technical innovation really happens through basic research, fundamental research programs, and not on huge scientific programs such as this.

Dr. **Trivelpiece**. I suspect that some of the knowledge that Professor Anderson used in his quest for activities that he was doing in the course of his research in Bell Labs was in fact built on knowledge gained through earlier high energy physics activities. It seems a little disingenuous to say that tools such as synchrotron light sources and so on are not going to be useful in providing the kind of information that he needs.

The **Chairman**. Finally, he says that:

> While this does not dominate physics employment, that high energy physics does dominate physics education, and that it is getting very difficult to find highly quali-

fied physicists in other areas of education because the best and the brightest of a very limited pool are being siphoned off, and particularly will be if we build the SSC, into high energy physics education.

What is your response?

Dr. TRIVELPIECE. I think the young people of our nation are smarter than that. They vote with their feet, regardless of what some of those of us in the grey-haired category of leaders in the academic community try to foist off on them. They know where the action is. They will go where the action is, unerringly. If high energy physics is where the most interesting and exciting problems are, whether the SSC is around or not, they're likely to be working on it. Certainly it will increase their appetite and it will create an attraction coefficient for it.

I think you have to look at how these events occur. I suspect that there are a large number of graduate students in the past few months who have decided that superconductivity, rather than being a somewhat arcane and uninteresting area, has suddenly become fascinating and they have now switched advisors or they have switched universities or they have switched activities in some way.

There is a certain drive on the part of smart young people to seem to pick out those areas that are most important. If high energy physics provides one of the vehicles that attracts them into science and they stay in that field, fine. If it attracts them into that field and they go work in some other field subsequently, that's fine, too. I think the nation wins both ways. It may be that one particular discipline of science doesn't win, but I'm not so concerned about that as I am concerned about trying to provide the impetus for the nation to have its young people pursue careers in science and technology.

The CHAIRMAN. I will return Dr. Anderson's statement to him with the correct comment.

Thank you, Mr. Chairman. [Laughter.]

Senator FORD. It is always good to be on this Committee.

Senator Hecht.

Senator HECHT. Thank you, Mr. Chairman.

I just have one fast question, and I'd like to yield the balance of my time to my distinguished colleague from New Mexico. Is that all right?

Senator FORD. That would be fine.

Senator HECHT. Dr. Trivelpiece, we have several spots in the State of Nevada that are under consideration that are actively seeking the SSC. What assurance can you give that the best site will be chosen? Many States are mounting a tremendous public relations campaign now.

How will you determine which State will be selected?

Dr. TRIVELPIECE. The first thing was this document which we prepared, which I hope you have a copy of, which is the Invitation for the Site Proposal (ISP).* It outlines the requirements that we believe necessary for making a proposal submission: namely, identifying the right amount of land, identifying the fact that electricity and water and other requirements to make a facility like this work, that those are there. In addition, it is part of the President's understanding of proceeding with this that we are to seek the maximum cost sharing. We provided in the invitation for site proposal a request that the States identify, or the proposers identify, how they would propose to cost share in this particular activity.

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*DOE/ER-0315, Invitation for Site Proposals for the Superconducting Super Collider (SSC), April 1987.

There is a site selection task force within the Department. That group was required to sign a conflict of interest statement and a confidentiality statement so that the integrity of this process is maintained, as it would be with any procurement process—we believe we're doing the same thing with the National Academies of Science and Engineering, in their evaluation. It is important to ensure that integrity is maintained so that those States who submit proposals have the assurance that they're being treated fairly and equally, and that the selection will not be the result of a publicity campaign, but a response to those things in the ISP that are required.

Senator HECHT. Thank you very much.

Pete.

Senator DOMENICI. Mr. Chairman, I asked Senator Hecht if he would yield because I have to go mark up, hopefully, with Senator Chiles and see if he can finish his budget work, and I will not impose on Senator Hecht's generosity with very lengthy questions.

Dr. Trivelpiece, since you are not going to be with the Department much longer, I first want to compliment you on the work you have done.

Dr. TRIVELPIECE. Thank you.

Senator DOMENICI. I think you have a very difficult job within the Department because of the nature of your work as compared with the defense work that it does. All the high physics research is very important to the future of our country and I commend you for what you have done.

There will always be some who are going to disagree, but you and others and the American Physical Society's patience won out, after years of debate, for the most part. The physicists of this country finally got together and said that they supported this, and only after that did you all take it to the President.

Frankly, the way we handle new programs that are mere authorizations here—and that is all this would be; this Committee would have no authority to fund this program—we put it on the table. If Congress passes it, then the process of competing with other programs and/or having to raise revenues or money to pay for it takes its normal course over the years.

The bill will not be subject to a point of order on budget grounds, because it is only an authorizing bill. If Congress does not want to appropriate later, they do not have to.

But the converse is not true, although it should be true around here. They should not appropriate unless a Committee like this has authorized. And we hope that is what this Committee is going to do, is exercise its jurisdiction in that manner and say yes or no.

I hope to say yes. Frankly, we have a lot of big science programs we are not sure we know how to pay for. We have—my recollection is we have a space station still planned, and if my recollection is correct that is $18 billion. It is on the agenda.

If you look out there and say they are working on the fundamentals for it, it is part of America's grand plan, but ask if it is plugged in in the fourth year out for all of the money that is needed, the answer is no.

I do not think we know exactly how things will turn in the next three or four years in terms of other programs, projects that we may not need as badly as this one, or what the revenue posture of the country will be. So I think this Committee ought to pass on whether this is a significant program for America or not.

I happen to conclude that it is extremely important that we maintain our lead in this area, and we are dropping quickly behind. And that has been your testimony before and I assume that is still the case, is it not?

Dr. TRIVELPIECE. That is correct.

Senator DOMENICI. I just have one question that does concern me about how we are going to pick the site. I am not at all concerned about scientists, the National Academy of Science, finding the best site or the best five or six sites. But one part does concern me, and I might call it sort of the "rich-get-richer" syndrome.

I understand that some States have put up a half a million dollars to do their planning, some a million, some two million. But I understand that some States are under the impression that, in addition to planning and site preparation and putting their own proposals together, that they think they will have a better chance if they offer the federal government $10 or $15 million as sort of their money on a $4 to $5 billion program.

What is your feeling about how important to a project of this type should that sort of up-front State money be—I am not talking about adequacy of site. How important should State cash contributions be in the site selection process?

And are there any decisions that have been made as to how important it will be?

Dr. TRIVELPIECE. There are two factors. We have tried to identify what are the technical site characteristics and the cost considerations. Both will be taken into account.

The complexity is that you have to look at the total cost over the life of the project. So there's a life cycle cost section in the invitation for site proposal that talks about how that will be taken into account. An analysis will be performed of what it will cost to run it for its entire, say, 25-year lifetime. That will be made available to those who are then doing the evaluation.

The President did, as part of his decision, have the understanding that we would seek the maximum possible cost sharing from the States and local governments. So this also means that there is a provision in here for the States to make known an in-kind contribution, or whatever.

Certainly if this facility were to go into a particular State or region, the benefits to that region would be substantial. The first and lowest level, of course, would just be the tax revenues to the State from the cash flow resulting from the construction funds and so on being spent there.

In addition, if you look at the benefits that have accrued to the region immediately around Fermi Lab—when that was sited there that was just open farm land. Of course, now it's a booming high technology region, with several companies having located their R&D laboratories there.

Who's to say why Silicon Valley exists, but it certainly exists in part due to the existence of the Stanford Linear Accelerator Center.

I think that the States that are competing for this have to take into account these long-term benefits in terms of trying to gauge what they believe is an appropriate level of cost sharing for them. I realize there may be a difference here between the Administration and the Congress. Our job for the nation as a whole is to try to get the best deal for the Federal taxpayers.

Each of the States, of course, would like to see these things modified in a slightly different way. I don't know how to take into account the kinds of concerns that you're expressing.

For us to suggest some limitation on this would be very difficult, because that goes against the general rule that we're going to try to get this at the lowest effective cost to the Government, including life cycle and so on.

Senator DOMENICI. Well, let me just tell you for the record why I object and why I think it is ridiculous. When we build a military base, when we build other federal facilities, there are generally very good benefits to an area. And I believe that if we are going to build a facility in the best possible site and that it is a national facility, that if we start on each and every major project saying, what will you pay for this federal project, I think we will have totally distorted the process of evaluating.

And I conclude that the rich will get richer and the poor will get poorer in terms of the States of this Union. I do not see how, if that is literally adopted, how most of the States in the Union that are really sites that have everything else that is necessary, how they are going to compete with a State that offers $50 or $100 million or $200 million.

And I really think in the scope of things that that kind of a cost sharing is insignificant. I think it has very, very little to do with where the facility ought to be and why our federal government is in the business of building it, at all. So I do not agree with the cost sharing aspect at all, and I do not think we have agreed on that kind of formula in this place, and we have to authorize it before we finish, I think, Mr. Chairman, do we not?

We have to put a bill out that says SSC is authorized. Then we can surely pass on some of the major criteria, either by prohibition or by some kind of language.

Thank you very much.

Dr. TRIVELPIECE. Senator, I think there might have been a slight misunderstanding. We are looking for the best site for the SSC. The combination of technical considerations and cost considerations are such that I don't believe that it is possible for this project to be bought in the sense that you're using the term.

If you look at the criteria that are in here, there is a balance between these things that I think addresses the concerns that you have. Perhaps, if you'd like some additional explanation on this, it could be made available.

Senator DOMENICI. Well, I would be pleased to have it.

I would just conclude that, if military bases are plusses to the economic environment, we ought to put them out to bid, too.

Dr. **Trivelpiece**. Some of them are.

Senator **Domenici**. Very few military establishments are located on the basis of how much a local community will pay for the barracks and the physical facilities or an airstrip. And if we did, I assume you would have plenty of sites that would all fit, under your last explanation.

But the point of it is, one State would offer $50 million for an air base and the other would offer $5 million, and the State that cannot do either because they are too poor, is a good site and part of the Union, and it just seem to me we are going to begin to Balkanize the whole process of major facilities, and this one is going to be the leadoff one.

Senator **Ford**. Senator Bingaman.

Senator **Bingaman**. Thank you, Mr. Chairman.

Let me ask on the report that you have got there in front of you. This is this central design group detail. Is this the conceptual design——

Dr. **Trivelpiece**. Conceptual design report, yes.

Senator **Bingaman**. When was that completed?

Dr. **Trivelpiece**. It was submitted to the Department in April of 1986.

Senator **Bingaman**. Now, to get back to the issue that was raised earlier by Senator Johnston about superconductivity, is it not true that since this report was done there really have been very major and basic scientific breakthroughs in superconductivity that have become generally known in the scientific community?

Dr. **Trivelpiece**. That's correct.

Senator **Bingaman**. And is it fair to say that the proposal that's being made to us now is being made without any detailed or systematic concern for those scientific breakthroughs?

Dr. **Trivelpiece**. In one sense yes and in another sense no. I mean, the proposal, the conceptual design report, is based on a sound sequence of engineering choices that include such things as reliability, manufacturability, the ability to produce conductors, and so on.

It's my feeling that the likelihood of these new discoveries being capable of competing on this time scale is that they won't. They may well compete at some stage in the future but I doubt over a 15-year period that that's really very likely.

Senator **Bingaman**. Where did you come up with the 15 years?

Dr. **Trivelpiece**. Well, if you look at niobium-titanium and niobium-tin, those were alloys that were considered. One of them is still very brittle and not very ductile. You'd have difficulty fabricating it into conductors.

You'd have even more difficulty making it into cables. You'd have difficulty making the cables fit on the mandrils that are needed in order to make these magnets. You're now talking about a material that is a glass, essentially, it's a ceramic. You need to somehow make that into a ductile conductor, make the conductors into cable, and so on. Once you've made these things, reliability tests have to be done, operating these things together in groups has to be done. There's a tremendous collection of engineering activities, none of which have been done beyond this laboratory discovery. I don't see those being done on a time scale that is consistent with building the SSC.

There are always surprises. I mean, that's one of the interesting aspects of science, both the discovery and technological breakthroughs. Certainly a fortuitous discovery could change everything I have said, but my judgment is that it is not likely to occur. Perhaps you should ask others who are also experts in this field as to what their opinions are.

But I don't happen to agree with Dr. Anderson.

Senator BINGAMAN. Well, I am not just referring to him. I am just wondering if the sequence that you are anticipating—and I am not in any way objecting to the project. I think it is important research that needs to be done.

But I guess I am questioning whether we should get some additional view from this same group that did that study and completed it a year ago, or if there is some other group that is more qualified to assess the impact of these new discoveries on the design of this facility before the Congress goes ahead and authorizes $4.4 billion.

As I understand it, the issue is whether you are going to use the conventional magnets that are helium—you have to use liquid helium to cool them?

Dr. TRIVELPIECE. That is correct.

Senator BINGAMAN. Or whether you can shift to some other kind of a magnet that would be cooled by liquid nitrogen, and the cost difference or something else. But the cost difference would be very substantial.

Dr. TRIVELPIECE. It would be a great advantage, no question.

Senator BINGAMAN. And also the size of the facility would be, could be significantly altered if it were possible to use these breakthroughs in the new facility; is that generally right?

Dr. TRIVELPIECE. As you have stated it, yes, that's correct.

Senator BINGAMAN. Well, I am just wondering if you should not—or maybe you have—try to get a reassessment of this proposed design in light of the new breakthroughs that have been made, even if that means a few more months of analysis.

This is a fairly long term project and a fairly expensive one, and I just wonder if the better part of valor is not to at least try to systematically reassess in light of these breakthroughs.

Dr. TRIVELPIECE. Well, an analysis could be done, but I do not believe the results would be conclusive because of the number of unknowns and variables.

The level of activity now in superconductivity with respect to the types of materials that'll be used in the SSC is 2,800 amps of current per square millimeter which is the current state of the art. The state of the art with the other superconducting materials now—and these have not been fabricated into large conductors yet—but it's around 1,000 amperes per square centimeter.

So instead of being a square millimeter, it's a square centimeter, which is about that big instead of being a tenth that big. Twenty-eight hundred amps per square millimeter is capable of going through the present generation of superconductors.

There is already some difficulty in going from that level, from one level to the other, of achieving what is already state of the art with the niobium-titanium materials.

Senator **Bingaman**. To shift subjects a little, are you proposing that Congress authorize funds to pay for this entire project, or are you suggesting, you the Administration suggesting, that significant contributions would be made by some of our allies?

Dr. **Trivelpiece**. I think the answer is yes to both. Yes, it would be desirable to have this project authorized in its entirety. It is also desirable to have cost sharing from other governments.

The cost sharing from other governments needs to be explored through regular diplomatic channels. That process has been formally initiated. We have not received any formal proposals yet.

Senator **Bingaman**. What would be wrong with the Congress authorizing the expenditure of a certain amount on this project contingent upon our allies coming forward and picking up some reasonable share of the costs, since this basic research is going to benefit them at least as much as it does us?

Dr. **Trivelpiece**. Well, this matter has been debated a lot within the Administration, and I think it's a bad idea. The reasons are that to enter into a negotiation in which you put in the hands of the other party the tool with which to determine what they're going to get from you I think is a bad situation.

I think I would advise against the United States being put in that position. I think the admonition is just as the President said, that you should proceed and you should seek the maximum possible cost sharing from foreign governments, state and local governments, and the private sector. That puts the burden on the Secretary to go out and do that. If he doesn't do a successful job at it, he shouldn't propose to proceed.

If you put a hurdle rate in, then that hurdle rate simply handicaps him in his ability to make a deal with any government, because then they know that they can hold out or that they can get from us what they want in order to put us in a position to agree.

I would hope you would not insist on putting in such hurdle rates, but putting in an admonition that we should seek the kind of cost sharing in the same manner the President suggests is absolutely first class.

Senator **Bingaman**. Well, it strikes me that we have got two different strategies here. When we go into arms control, the theory is that we should go in with a tough bargaining position and Congress should see to it that the Administration has as much leverage as possible at the table.

And here we are saying, let us go ahead and authorize the expenditure of U.S. tax dollars for the entire project, and then send our people in to negotiate with the Japanese and ask them to pay for part of it. It does not seem to me to make a lot of sense.

Dr. **Trivelpiece**. I submit that under the present circumstances, the way I suggested is the way we have the maximum possible leverage, that this is a highly desirable activity, that some of the other countries are going to want to participate, and that we should see what they're interested in doing and determine what they expect in return, and see whether or not that's a suitable situation for the United States.

That is, I think, a stronger bargaining position than the other way around. The SSC project is something desirable, not something undesirable. I think perhaps what you say in connection with the other kinds of negotiations might be a proper initial strategy, but I don't believe so here.

Senator **Bingaman**. What is the possibility that some of our allies, either the Japanese or the Europeans, would let us go ahead and build this as it has been designed and then come along within a few years, using some of these breakthroughs in superconductivity, and build a more powerful machine at substantially less cost?

Dr. **Trivelpiece**. There is an expression: Never say never. There are a number of people who have made very profound statements about what won't happen, like Vanever Bush saying that there will never be intercontinental ballistic missiles, and a famous British scientist saying that energy from the interior of the atom will never come about.

I would never say that that could never happen. I am highly skeptical, however, that that is a likely outcome from what I know about the situation. It is certainly within the realm of possibility that that could occur, but highly unlikely.

Senator **Bingaman**. Thank you very much, Mr. Chairman.

Senator **Ford**. Thank you very much, Senator.

We have a vote on, and we still have Senator Evans, Senator McClure, Senator Wirth, and Senator Bumpers to question the good doctor. So Senator Evans, I am going to ask you to go next and I am going to head over to cast my vote and then hopefully get back.

But Dale, you and Senator McClure can handle it until we get back.

Senator **Evans**. Thank you, Mr. Chairman.

Dr. Trivelpiece, I guess that we are all of a similar mind on the Committee, just listening to my colleagues' questions beforehand. Would it not be accurate to say that a year ago, if we had been sitting in this same room asking these same questions with the same proposal in front of us, nobody would have asked the question about superconductivity breakthroughs?

Dr. **Trivelpiece**. I doubt it, unless they were very prescient.

Senator **Evans**. So we are not at all certain that a year from now, if we were sitting here debating the same subject, that we might be at the point where we had some very clear ideas about the potential of this changing radically the design of this device?

Dr. **Trivelpiece**. Possible, not likely.

Senator **Evans**. You say not likely. But I guess then it comes down to sort of a fundamental question: How unlikely compared to $5 billion? Or what would or could the cost differential be if magically tomorrow we had superconductive material that could operate at room temperatures and change whatever nature or size of the device?

What are we really talking about anyhow, in terms of the cost?

Dr. **Trivelpiece**. Well, I'm not sure I would know exactly how to answer the question of how it would change the cost. That discovery would be of such momentous importance that something like the SSC would pale in comparison with it in terms of the transmission of electricity, the manufacturing of large electrical generating systems, transformers.

It's just hard to calculate exactly what, or even estimate what, the importance of that particular discovery would be. But it would clearly change——

Senator **Evans**. That is what we are aiming toward with what is going on now, however?

Dr. **Trivelpiece**. Yes, if the fondest hopes and expectations of those who are pursuing that area of research are realized, that is likely to be one of the outcomes somewhere down the road. But they first have to go from a ceramic material to something that's at least ductile and fabricable into materials.

Senator **Evans**. Sure. It seems to me that in the nature of science, however, just that is mechanics compared to the discovery to a degree. Once you know what you are after, you can figure out how to do it, that is a difference, that is a considerable difference than not knowing what to do or what you're going to find.

If that really would make this pale into insignificance, maybe we ought to spend $4 or $5 billion hustling that up, rather than putting it in this device.

Dr. **Trivelpiece**. As nearly as I can tell, almost everybody that can spell "superconductivity" these days is beginning to work on it. It's certainly true in the DOE laboratories. As I said earlier, I think we have something like $15 or $20 million going into this on an annual basis.

It's not even a question of our encouraging this and trying to make it occur. I suspect we couldn't prevent it if we wanted to.

Senator **Evans**. And you do think that if we were to pass a bill this year with the full cost authorization for this device, and proceeding ahead, locking in the engineering as of our knowledge today, you think that that would be a better investment than waiting any length of time?

Dr. **Trivelpiece**. I don't think that it is an either-or situation. I think if you were to do as you suggest, that should the kind of technology come along that would make a better way to go about doing this, that it would be adopted immediately. Whether or not that would change the authorization cost or not would remain to be seen. I suspect if there's a better way to do it and the project hasn't already been completed to the point that the magnets are built and installed in the tunnel, that any new technology would be utilized.

I call your attention to the subject we discussed in the past, the Continuous Electron Beam Accelerator Facility [CEBAF]. That literally had reached the point within the Administration that we had a validated project. We were asking the OMB for money. At that stage it became clear that use of superconductivity in the RF cavity structures would make a tremendous improvement in the project. Even though there was a substantial risk in terms of the departmental-OMB interaction and budget strategy, we went ahead with changing the project and getting it revalidated with the superconducting cavities. It did not result in any reduction in cost, but it clearly resulted in a substantial improvement in the capability and operating characteristics of that accelerator. I think it was an important thing to do.

So, it isn't necessary that you make this an either-or choice. I think if you proceed now and a major technological breakthrough occurs in time that it can be taken into account, that people working in this field would do so almost immediately.

Senator Evans. If you were to have an authorization tomorrow, how much is that lead time between tomorrow and the time that you would have to have a locked in design which could not be changed?

Dr. Trivelpiece. I hesitate to answer, but it's certainly not 6 months or a year. I mean——

Senator Evans. Well, certainly you have estimates in here about how long a period of time it would be. You have all of these nice charts with the time frames. Using those same time charts?

Dr. Trivelpiece. Well, somebody would have to go out and procure the wire for the magnets, develop the prototype winding machines. Some magnet prototypes have been built. They need to be operated in systems and so on.

This is a conceptual design report. This is not a detailed design. So from this conceptual design, there is some substantial period of time to make a detailed design. And that requires the hiring of architect-engineers, additional engineers to design magnets and make the prototypes, get them into industry, and get them manufactured. All of this cannot be done overnight, and I suspect we are talking about a couple of years.

Senator Evans. So a couple of years. So that is about the time that you have got in here for final site selection. Once a final site is selected, does that help lock in what you're going to do?

Dr. Trivelpiece. The simplest thing you might assume is that what would happen is it would still be the same magnetic field strength and the same energy, 20 TEV on 20 TEV. That's what determines the circumference and the general ring—shape of it.

That would be a question then of simply replacing one magnet type with another magnet type. If the project had not gone too far, that possibility would certainly exist, to make magnets that, instead of operating on helium, are operating on nitrogen, and are built of this new material instead of being built of niobium-titanium.

Senator Evans. Would this change the diameter of the total structure?

Dr. Trivelpiece. Not if it were the same magnetic field strength.

Senator Evans. Let me turn just very briefly, because a vote is coming——

Senator Bumpers. You are now the chairman, Senator Evans.

Senator Evans [presiding]. Let me just turn to the cost consideration. Let me say first that I thoroughly endorse what the Senator from New Mexico said. In fact, I would find it exceedingly difficult, in spite of the fact that I am a fan of basic research being done by the federal government and basically a fan of this proposal——

Senator Bumpers. Senator Evans, if you would yield, I think I have a solution to that. I think we have to offer this to any State that is willing to take a high level radioactive waste site, and put them together. [Laughter.]

Senator Evans. Let the ring glow in the dark.

Dr. TRIVELPIECE. I would hope they would not have to be colocated.

Senator EVANS. No, no.

In looking at the cost considerations and the technical evaluation criteria and all of the things that are set forth in this booklet, which is now being transmitted, as I understand, to the governors of all of the States, it says:

> For each proposal meeting the qualification criteria, a life cycle cost estimate will be prepared for the construction phase plus a 25 year operating phase. Any financial or in-kind contributions offered by the proposer other than the cost of land will be considered as appropriate in the LCC estimate. The cost of land of the proposer will not be considered.

And then it goes further down. It says:

> NAS, NAE, and DOE in their overall evaluations of proposals will consider whether a higher technical evaluation is worth the extra cost of a particular proposal and whether the LCC of a particular proposal is low enough to overcome a lower technical evaluation.

That sounds an awful lot to me like bidding. And I do not know whether you presume to give the initial evaluation—I can conceive a situation where you rank the States or sites in order, the top two are pretty close, but one is selected and the second State comes in and says: Well, according to your criteria if I bid another $50 million then that consideration makes me first.

The number three State says: Well, heck, I can outbid that; I will come in with $100 million. And under your qualification criteria, which I presume at that point will be all laid out on the relative merits and costs versus technical consideration—and the response to that last sentence I just read will have to be put in numbers, I presume, or some rational consideration—you are saying in essence it is out for bids, one way or another.

And I think that is wrong. In fact, I will not support this proposal if that is part of the consideration, and I do not think many of the colleagues on this Committee will either.

Dr. TRIVELPIECE. I am not quite sure how to answer your question. Certainly if it were technically unacceptable, if a State or location——

Senator EVANS. I am talking about two places that are relatively acceptable.

Dr. TRIVELPIECE. And both are excellent, otherwise excellent?

Senator EVANS. Presumably. One is technically better than the other, but both are acceptable.

Dr. TRIVELPIECE. I mean, if they're both technically acceptable and otherwise identical, then what criteria would you have the Department use in order to select?

Senator EVANS. I said one may be technically better than the other, but both are acceptable. Now, that is certainly feasible.

Dr. TRIVELPIECE. Well, this process is dedicated toward trying to identify a handful of those that are truly outstanding, excellent technically.

Senator EVANS. All right. You have five that are outstanding, but they are all relative. You have got all of these technical considerations.

Dr. TRIVELPIECE. Yes.

Senator **Evans**. You have ordered the priority of each of those. Presumably, when you come out the other end you will have some relative merits of each of those sites, even though they are all outstanding. One is more outstanding than another.

Then what do you do when the second State says, well, I will be prepared to bid more and give you the cheaper operation?

Dr. **Trivelpiece**. Well, except that there is not going to be a best and final after. Whatever is laid on the table as of August 3rd will be the final determinant. The Government has the right to proceed at that point without further contact with the States.

Senator **Evans**. Sure.

Dr. **Trivelpiece**. There is not a bidding.

Senator **Evans**. Sure they have the right. But the very process that you are talking about here contemplates getting the best deal for the government. You said that yourself.

Dr. **Trivelpiece**. Yes, sir.

Senator **Evans**. The best deal for the government would be to open it to rebid, get a better deal, keep getting a better deal until somebody quits bidding.

Dr. **Trivelpiece**. No, no.

Senator **Evans**. It is sort of like a Van Gogh painting that just went.

Dr. **Trivelpiece**. No, it's not an auction.

Senator **Evans**. Well, it certainly sounds like an auction.

Dr. **Trivelpiece**. It's a response to an invitation for site proposal. States are in a position to identify a piece of real estate, the fact that there is electricity, the fact that there is water, the fact that there are all of those other things that are physically available.

Senator **Evans**. I understand all that.

Dr. **Trivelpiece**. Those matters will be weighed off one against the other. In addition, the President said that we should try to seek the maximum possible cost sharing from the State and local governments. So we believe that we need to include that as a provision in the request.

We've tried to do so in a balanced way that takes into account both the technical considerations and the cost considerations and the life cycle cost matters. Indeed, it is likely as you suggest, that there will be a number that will be quite technically excellent, and it will come down to some small differences in the end.

And I don't know any other way to make a competitive procurement like this come out. Those are all the factors that have to be taken into account. Cost is not the only factor, but it is certainly one that has to be included.

Senator **Evans**. Well, then I would suggest that that be made very clear, and all but about four or five States might as well pack their bags, because they simply cannot compete under those circumstances.

And I think, given that, frankly I would just say that it might be a good word to take back to the Department of Energy that this program is in real trouble, because I do not think—there are an awful lot of supporters out here coming from States that simply will not be able to compete under that circumstance, no matter how good the site is technically.

Dr. TRIVELPIECE. Well, I certainly hear the message, Senator. I will take that back.

Senator EVANS. OK.

Dr. TRIVELPIECE. There's no misunderstanding.

Senator MCCLURE [presiding]. I would pick up on that last note something that has been kind of echoed and re-echoed throughout all of this, and that is right now there are a heavy majority of 50 States in favor of this project, because many of them think they are in the running. That broad national support is going to evaporate once the site selection is gone, or it will at least be greatly diminished.

Is the Administration going to ask that we now authorize and appropriate for the entire construction cost or just the beginning cost?

Dr. TRIVELPIECE. Well, I'm not familiar with the process by which the Congress appropriates for more than 1 year normally. I guess they can, but I don't believe that's the normal cycle.

Senator MCCLURE. It is not normal.

Dr. TRIVELPIECE. Certainly no request for that has been made. The out-year budget profile has been identified in the construction data sheet and we would believe that it would be desirable to have the entire project authorized. I'm not familiar, however, with the possibility of multiple year appropriations. It would be nice, but I certainly wouldn't hold out for that.

Senator MCCLURE. My concern is exactly what Senator Evans just identified, that once States are out of the running their political support for this project will no longer be there. And we may well see us getting into this process up to the point of making an award to a State, making a site selection, and immediately find that no longer is there a consensus here that this project has priority.

Dr. TRIVELPIECE. I would hope that the Congress collectively would take a higher plane view of this thing; there are some hundred universities throughout the United States that would use the facility. Those universities are located in most of the States.

This project really is something that will benefit the United States collectively, regardless of where it is physically located. There are certainly corporations that will do a great deal of the business that is involved in it. Those aren't all going to be located within 50 feet of the site boundary within the State.

I don't know how to define exactly what the distribution function of the benefits is going to be. But this is something that the site selection process should not get caught up in, the distinction of being a local benefit for local reasons, and the need for this facility for the United States for basic research and our general economic health and wellbeing.

I know that at times politics makes this difficult to do, but I would hope that there would be a recognition of the global importance of this, regardless of which State ultimately is the winner, and that the support would not evaporate, as you suggest. But I am realistic. I suspect that indeed a good fraction of it would.

Senator MCCLURE. Well, I certainly hope that does not happen. I have tried to suggest as an example that in the Inter-Mountain West the States that hope for its location there—that includes all of them, I think—join together in a regional compact, in effect each making an agreement with the others as to what their contribution would be in the event they are selected as the site and what their contribution would be if an adjacent State is selected.

Now, that is obviously for a variety of reasons, one of which is to strengthen the application, because a regional application in sparsely settled States would make a stronger application than would a single State application.

And secondly, it then gets a binding commitment for the region that, if each State drops out of the bidding, you lose the political support; but if each State stays in, even if they are not selected, then you broaden the base of political support as well as economic and financial support for the project.

I think that is very important, because I think as we look at this budget there are a series of questions, which I will submit for response in writing, that ask you to array priorities, this project versus other priorities. As an example, the basic physics research that is being done at many universities across the United States, and there is federal support for that.

You know, the aggregate amount of money now being spent by the federal government in such research?

Dr. TRIVELPIECE. Basic research, I think the number is generally reckoned to be around $6 or $7 billion, that part which is research. I mean, the total R&D budget for the United States, both industry and federal government, is something in excess of $100 billion.

It is roughly 50–50 Federal and private, and of the Federal most of that is development work and $6 or $7 billion of it is regarded as basic research, part in the Department of Energy, part in the National Science Foundation, part in HHS.

Senator MCCLURE. And that is very important for the health and the future of our country, is it not?

Dr. TRIVELPIECE. I like to think it is the most important part of that R&D budget, personally.

Senator MCCLURE. And at some point we may be asked to array this expenditure against that expenditure as a priority in our budget. That debate will focus much more sharply, once a site selection has been made.

Dr. TRIVELPIECE. Yes.

Senator MCCLURE. Because there will be different winners and losers.

Dr. TRIVELPIECE. Absolutely.

Senator MCCLURE. I have a number of questions along that line which I will submit, because I think it is important for us to get your opinion as to the relative priority of this project as against others.

It is obvious from the budget submission that you believe that the budget is appropriately balanced, and I think that is a given, is it not?

Dr. TRIVELPIECE. Yes, sir.

Senator MCCLURE. When it comes to local cost-sharing and you are looking at life cycle costs, such items as up-front site selection costs, site costs, are obviously there. If one place provides the site free of cost, another does not, the government has to spend money to clear that site or obtain the site, that is a consideration in terms of the contribution up front, is it?

Dr. **Trivelpiece.** No. The expectation is that regardless of the circumstance by which the State comes to take title to the land and deliver title to the Federal Government, that that is done without cost, whether they have to pay for it or whether it is preexisting State land or whatever.

Senator **McClure.** So every State should be forewarned that they are really not in the running unless they can provide the site cost-free?

Dr. **Trivelpiece.** That is correct. That is clearly stated in the Invitative for Site Proposals.

Senator **McClure.** The second question with respect to life cycle costs and cost sharing would be the issues about whether or not there are taxes to be paid. If one State levies taxes and another does not, that makes a difference in life cycle cost.

Dr. **Trivelpiece.** Clearly.

Senator **McClure.** And you would calculate—let me reverse that. How would you calculate the local cost sharing with respect to revenues foregone?

Dr. **Trivelpiece.** You're getting me onto dangerous ground, although I think that's a perfectly legitimate question. I don't want to do anything that would compromise the integrity of the site selection process, so I would like to, if possible, supply that answer for the record if I can; and if I can't, then give you the reasons why. But I would prefer to talk to our procurement folks before answering.

[The information follows:]

Local Cost Sharing

Revenues foregone would not be considered as part of the life cycle cost calulations, i.e., as a contribution. This is because the land must be offered to the Government without cost. This puts all proposers on the same basis for all life cycle cost analyses.

Senator **McClure.** All right. Let me focus a little bit on the kind of thing I want you to have in your mind when you answer that question and supply the answer for the record. One State that provides the site to the federal government without cost may acquire the land and place it in a nonprofit corporation or by whatever means they might do it. Some, they will acquire title to the land in fee simple. Others would acquire easements which from the federal government's standpoint are just as good if they can operate, I assume, but might have different consequences with respect to local property taxation.

Those of us who come from public lands States will look at land already owned by the federal government. There is no cost of acquisition, so we would start out even with a State that would acquire cost-free for the government.

But I would not want to see a public lands State lose out in the mathematical calculation because they do not levy taxes in a State where it is not federal lands and therefore have that revenue foregone in local taxes counted as an in-kind contribution or a local cost share, as against a State where it is all federal land to start with and no tax would be levied on the land.

Dr. TRIVELPIECE. I understand your question. I would still like to answer it in the same way.

But the point—it doesn't necessarily mean because it's federal land that it is free of charge to DOE. If it happens to be under the ownership of another Federal agency, GSA requires that land not be transferred from one federal agency to another without its fair value being paid for it.

And in the most recent case, the CFBAF accelerator down in Newport News, the City of Newport News gave us the $2.3 million to pay to GSA because NASA was the Federal agency that owned the land.

It doesn't always follow that because it's Federal land that it is not without cost to somebody. There is a variety of things, whether it's federal land or public land or State-owned land. It's quite complicated, and that's the reason that we elected to choose the strategy which we did. It is the obligation of the State to provide the land to the Federal Government in fee simple title, without encumbrance.

Senator MCCLURE. And that is not counted. That is not counted a contribution.

Dr. TRIVELPIECE. That's not a contribution. However the State comes by this land, it is expected to be made available for this purpose.

Senator MCCLURE. And with respect to transfers between federal agencies, we already have a half million acre reserve out in Idaho that is already dedicated to the Department of Energy, and I assume that puts us miles ahead of the competition.

Dr. TRIVELPIECE. I'd prefer not to answer that question, Senator. [Laughter.]

Senator MCCLURE. That is all right. We already have that question solved for us.

Obviously, there are other things, such as the availability of water, the availability of low cost power. The State of Idaho will compete with anyone. I think it is a superior site in that regard.

And with respect to local labor—and I think that is an important factor—I know when we were dealing with the question of a new production reactor, the local trade unions entered into a compact with the federal government: By public declaration, there would be no work stoppages during the construction of the project.

They demonstrated in the building of the chemical plant that they have the competence of a labor force to build anything of a technical nature as well as anybody in the United States can. And I know that is a part of your site selection criteria.

I commend you for a number of the things that you have said in your site selection criteria, because obviously I think Idaho competes very well. But I also recognize the difficulty that Senator Evans referred to on the ability of a small State to come up with local cash contribution. I do not want to see us get into a bidding war.

I do not think that that is really what you have in mind, but I do understand what OMB might do with respect to life cycle costs driving us toward whatever kind of deal yields the least cost to the Federal Treasury. And sometimes that leads us to make penny-wise, pound-foolish decisions.

I hope we can thread our way through that without any great difficulty.

Dr. Trivelpiece, you mentioned a moment ago in response to a question of Senator Bingaman with respect to the breakthroughs on superconducting materials that you did not believe that this required or indicated any desirability in delay. Did I understand you correctly?

Dr. **Trivelpiece**. That is correct.

Senator **McClure**. If I read the material that has crossed my desk at all correctly, while there have been breakthroughs and the promise is very great, there is still a great deal of experimentation and verification before we will really know what that means, is that not correct?

Dr. **Trivelpiece**. Yes. Nobody has come forward with a ten foot long piece of wire that is homogeneous and can be tied into a square knot sharply and not break. When that happens, then I think we may want to revise our plans.

Senator **McClure**. And we do not really know when that will occur, do we?

Dr. **Trivelpiece**. No. And it is very difficult, when you consider, as I said, that the material itself is a ceramic-like material, how to convert that into a suitably ductile material that can be fabricated into wires remains to be seen.

Senator **McClure**. And you think that that process of authentication and verification of results and the development, the time that it takes, is greater than is likely—is likely greater than we could accommodate with respect to delay in the design and construction of this project?

Dr. **Trivelpiece**. Absolutely.

Senator **McClure**. And it is for those reasons you suggest no delay?

Dr. **Trivelpiece**. Yes, sir.

Senator **McClure**. One of the questions asked with respect to design is if there is any justification in building a wider tunnel, so that adaptations might more easily be fitted into that tunnel, than the one that is designed?

Dr. **Trivelpiece**. Certainly a ten foot diameter tunnel allows for quite a few things to be put in there. I don't know what the differential costs per additional foot of diameter is, but it's obviously going to be reasonably expensive a very large tunnel. It involves the volume of the earth you remove and the circumference of the tunnel.

Those factors would add costs. I just don't know what the relationship is. I don't know that anybody's even explored it. We could take a look at that.

Senator **McClure**. I wonder if you might do that and give me some kind of an answer for the record.

[The information follows:]

The architectural/engineering firm, RTK, that supported the Central Design Group in developing the conceptual design for the SSC examined the question of differential costs associated with larger tunnel diameters. They used material prepared by the National Committee on Tunneling Technology of the Academy of Engineering. By studying the costs for completed tunnels, the associated physical dimensions, and methods of construction, RTK developed costs that can be used to relate cost per linear foot to the finished inside diameter.

RTK looked at a variety of tunnels in the vicinity of 10 ft. inside diameter (12 foot outside diameter with a 1 ft. thick liner) excavated through hard rock. They derived an average cost from the existing data. Tunnels that were 3-4 feet larger in diameter cost about 40 percent more per linear foot. Tunnels that were somewhat smaller followed a similar pattern. Consequently, for hard rock tunnels the incremental/decremental cost for larger or smaller tunnel diameters in the range being considered is about 10 percent per inside diameter foot.

Senator McClure. I recognize that there are a lot of tradeoffs involved, and certainly costs escalate geometrically as volume increases by diameter. But this is going to be a pretty large construction project just for the tunnel itself, and we would not want to come back later and say: Well, we have constrained ourselves too much and now we cannot get in that tunnel what we need to do, and we have discovered something new that we would like to run another one along side the other one, and we do not have room to do it and we have to go build another one somewhere.

Dr. Trivelpiece. Although I've never built an experimental facility of this scope, in every experiment I've ever built, one of the principal dilemmas, has been putting enough ports on it, making it big enough, making the field strength a little bit bigger.

Eventually you run into cost tradeoffs you just simply can't digest. I don't know what the cost tradeoffs are in this case. What you suggest is a good idea and does allow for future possibilities, but I don't know whether the initial costs would be prohibitive.

Senator McClure. What events led to the cancellation of DOE's Isabelle accelerator?

Dr. Trivelpiece. That's a very painful story.

Senator McClure. I am sorry about that.

Dr. Trivelpiece. One of the things that I had to do literally almost on the first day I arrived at the Department of Energy was review the Isabelle project. I came to the conclusion that the likelihood that it was going to make it was very small and I withheld about $60 million in 1981.

Subsequently, the laboratory, Brookhaven, did a good job of trying to engineer their way out of the problems. The civil construction had gone ahead very smartly, a $119 million tunnel was built. It's still up there in Long Island. It's empty.

When it came time to put the magnets in the tunnel, the magnet prototypes didn't exist. The manufactured magnets didn't exist. The magnets hadn't been designed.

In reviewing the whole process, it became clear that there was quite a bit of difficulty. I terminated the project and at the same time asked that the high energy physics community—and in fact, this is the report here. I asked them what are the next steps, what should be done. That's what led to the recommendation that we proceed to the SSC.

Now, there are many lessons to be learned in the termination of the Isabelle program. One of them was that you need to carefully plan and make sure that there are no technological hurdles in the project, that you know what the costs are, you know what the schedules are.

That's why I believe that, because of that, I was extremely cautious. I wasn't about to go forward to the Under Secretary or the Secretary or anyone else until I was satisfied that this particular project met those criteria. So for the last three years we've spent about $20 million a year through the central design group, and we've come up with this collection of reports.

I believe that this is probably the best analyzed activity of this sort ever submitted by the Administration to the Congress. We know within a very narrow range what it's going to take to do it and what the difficulties are.

Many of the cost-reducing design assumptions were in fact carried out experimentally. The magnets at Fermi Lab have one level of heat loss. In order for this to make the cost limitations that were thought to be necessary, something like one-fifth the heat loss was taken into the calculation.

So magnets were in fact built and it was demonstrated that the heat loss could be reduced by a factor of five in the prototype design. So several experimental activities were carried on in conjunction with the paper studies in order to verify some of these things.

It is in part the difficulties with Isabelle which have led to probably a much greater than average depth of analysis in connection with this SSC proposal.

Senator McClure. So the lessons that we should learn from Isabelle have been not only learned, but applied?

Dr. Trivelpiece. They are almost ingrained in my soul. There is hardly a situation that comes up in the Department of Energy in which someone doesn't say: Well, we don't have another Isabelle here, do we? They're not even sure what that reference means any more. What it means colloquially within the Department is that a project had better be carefully analyzed and documented before you bring it forward and seek any kind of approval from the Secretary.

Senator McClure. Thank you very much.

Mr. Chairman, I ask unanimous consent that an opening statement on my behalf be inserted at an appropriate place.

Senator Ford [presiding]. Without objection, so ordered.

And I appreciate the Senator looking after the interests of the Committee.

Senator McClure. Certainly. And could I also ask unanimous consent that certain questions submitted for response in writing be submitted?

Senator Ford. Without objection, it is so ordered. And I am sure the doctor will respond in a timely way. Before you leave, Doctor, can you get it done?

Dr. Trivelpiece. We'll do it. I'm not leaving until the 24th.

Senator Ford. Oh, fine.

Senator Melcher, do you have any questions of the doctor?

Senator Melcher. No, I don't, Mr. Chairman.

Senator Ford. All right.

Doctor, we thank you for your patience. You have been here for an hour and a half, and we appreciate it very much. And there will be some questions in writing to you. And best of luck to you.

Dr. Trivelpiece. Thank you, Mr. Chairman. It's always a pleasure.

Senator Ford. We look forward to you coming back and visiting with us.

Dr. Trivelpiece. I hope so.

Senator FORD. Dr. Betsy Ancker-Johnson is the next witness. And doctor, I understand you have a time problem. And I apologize for that. What I think we will do here, if it suits you, is that we will let you make your complete statement, and if we have any time left for questions we will, and have those limited.

I understand you have to leave before 4:00 o'clock, is that correct?

Dr. ANCKER-JOHNSON. At 4:15, sir.

Senator FORD. Well, OK, fine. That gives us a little bit more time then.

You may proceed.

STATEMENT OF DR. BETSY ANCKER-JOHNSON, CHAIRMAN, PHYSICS REVIEW BOARD, ENERGY RESEARCH ADVISORY BOARD, DEPARTMENT OF ENERGY

Dr. ANCKER-JOHNSON. Thank you, sir.

I had planned, if it suited your convenience, Mr. Chairman, to abbreviate my oral statement and you have it for the record.

Senator FORD. Absolutely. Your total statement will be included in the record.

Dr. ANCKER-JOHNSON. Thank you, sir.

I'm here, Mr. Chairman, because I chaired the Physics Review Panel of the Energy Research Advisory Board, which has just completed its work on the National Research Council report, "Physics Through the 1990's."

Physics research is vital to the long term development of advanced technology. One may convincingly argue that the United States still maintains a lead in many fields of physics and in much of the advanced technology development stemming therefrom, but the trend may well favor our competitors.

It thus behooves the nation to consider very thoughtfully how much it spends on physics research, both basic and applied. Physics, the science of the most fundamental aspects of nature, has had a profound impact on our view of the universe and on the manner in which we lead our daily lives.

Almost every aspect of modern society, including manufacturing, transportation, communications, and health care, has evolved directly or indirectly from the discovery and exploitation of fundamental physical phenomena. In particular, physics is involved in almost every aspect of research on the generation and efficient use and transport of energy.

In recognition of the enormous importance of physics to the economic, material, and intellectual wellbeing of the country, the United States Government, largely through the Department of Energy, substantially supports many areas of physics.

The DOE provides about $1.2 billion annually to support research in basic and applied physics, a sum that represents over half the national total. In certain sub-fields, elementary particle, nuclear, and plasma physics, the so-called big science fields or big physics fields, it is by far the major supporter.

In the so-called small physics fields—condensed matter physics and atomic, molecular, and optical physics—DOE's support is also indispensable.

In 1983, NRC initiated a comprehensive survey of the state of physics in the United States. The physics survey committee was chaired by Dr. Brinkman of the Sandia Laboratories and involved nearly 100 distinguished scientists with expertise in the different areas of physics, as well as the staff of a number of agencies and societies.

They finished their report in May 1986 and we were charged by the Department of Energy to make a survey of that, or to report on it. While the NRC report highlights many impressive intellectual and practical achievements of U.S. physics in the past ten years and identifies many exciting opportunities in the near future, it also finds certain problems and issues facing the discipline today.

Our ERAB panel gave careful consideration to these questions in formulating the following six recommendations. A major concern expressed in the NRC report and strongly shared by us is the unsatisfactory condition of basic physics research in universities. We recommend that DOE provide new funds for research and instrumentation to a number of small academic research groups performing research of high quality.

Our panel was asked in its charge letter to keep in mind the unique role that the DOE has in funding and managing major physics facilities. It's clear from the information received by our panel that nearly all existing facilities are being operated at levels which fall far short of what is scientifically optimal.

We recommend that appropriate funding be provided to assure an adequate level of operation of these facilities and to upgrade the associated research instrumentation.

Our panel recognizes the great scientific importance of the Superconducting Super Collider, which will enable experimental exploration of key unresolved issues in elementary particle physics. We understand the virtually unanimous support for this facility in the high energy physics community.

At the same time, the magnitude of the project means that it cannot be undertaken without a multi-billion dollar incremental commitment to basic science over the next decade. The Department of Energy Research Advisory Board's policy on this question is stated in a report preceding ours.

This policy is that the SSC should be funded subject to the requirement that other R&D priorities within DOE not be preempted. Our physics panel supports this position and recommends that funding for the construction of the SSC be considered in a national context which is larger than that of DOE alone and independent of the facilities whose construction we recommend.

Now, as to those facilities for which we recommended construction be undertaken, these four have been identified by the respective user communities as being of paramount importance. I list them here in the order of their proposed starting dates:

First is the Compact Ignition Tokamak. This facility, whose construction is enthusiastically supported by the Department of Energy Research Advisory Board Magnetic Fusion Panel, is designed to perform a deterium-tritium ignition physics test, study confinement and control of a burning tokomak plasma, and provide a data base for design and engineering a test reactor.

It represents a logical and cost effective step toward a commercial fusion reactor.

The second device is a 7 GeV advanced photon source. This facility will generate X-ray beams of unprecedented intensity for up to 200 simultaneous experiments. Its scientific and technical applications include advanced materials, semiconductors, lithography, catalysis, geology, medical imaging, bioengineering, and structural studies of key biological systems, such as viruses and proteins.

The third device or major facility is a relativistic heavy ion collider. This accelerator will produce very high energy densities in nuclei, sufficient to lead to the formation of a quark-gluon plasma, a unique form of matter that has not existed since the first instants of the universe.

And the fourth is the advanced neutron source. This research reactor will provide the most intense beams of steady state neutrons in the world and a wide array of cold, thermal, and hot neutron beams. It will enable the United States to take a leading position in the application of neutron techniques to colloid and polymer science, biology, chemistry, metallurgy, and ceramics, as well as to the physics of solids and fluids, and to nuclear and fundamental physics.

Our panel emphasizes that the financing of new facilities should not be allowed to undermine the basic research programs on which the achievements of the new facilities will depend. We are particularly concerned about the steady decline in the support of plasma and fusion physics over the past few years and recommend that future budgets reflect the need to maintain a balanced program.

The NRC report raises concern about the education and training of physicists for the nationally important programs of the future, and we recommend that DOE address the potential imbalance between supply and demand by initiating fellowship programs at both pre-doctoral and post-doctoral levels.

Mr. Chairman, our panel estimates that the cost of carrying out the above recommendations will require an increase of approximately 50 percent in real funding for basic physics research in DOE between Fiscal Year 1986 and 1992.

We firmly believe such an increase to be justified by the great importance of physics to the programs and objectives of the DOE and the United States. In the provision of economically attractive supply and efficient end use energy options, the mitigation of the health and environmental impacts of these technologies, and the advancement of fundamental scientific knowledge, the different sub-fields of physics are playing an increasingly vital role.

The recent discovery of ceramic materials that you've been hearing about and discussing here a good deal today, that remain superconducting at liquid nitrogen and even higher temperatures, is just one example of the great potential of new concepts being explored.

The phenomenon of superconductivity at readily accessible temperatures will have a truly profound effect on science and on industrial technology. Moreover, the pace at which new superconducting materials are emerging from so-called small physics laboratories suggests that commercial applications may appear even faster than was the case when semiconducting materials were discovered.

Physics research has had profound commercial applications. For example, ongoing developments in microelectronic devices and computer components may ensure our nation's economic viability in the twenty first century. Furthermore, although not explicitly included in our charge or in the charge to our panel, basic physics research is essential to the nation's defense capabilities.

The returns to the nation from its investment in basic and applied physics research has been tremendous, and the future potential is even greater. I urge the Members of the Committee to continue their strong support of this vitally important field.

Your problem currently is to decide whether to fund the SSC, or to authorize it. I hope you will, but I urge you not to do so at the expense of the rest of physics research.

And finally, speaking now solely for myself, I urge you not to fund construction of the SSC until a thorough assessment is prepared about the applicability of these relatively high temperature superconducting materials to the SSC design.

Should a synergy develop between high energy physicists 'goals and those of condensed matter physicists which hastens the application of the latter's current superconductivity research, everyone in the U.S. would benefit because of the enormous commercial promise of superconductivity.

Thank you.

[The prepared statement of Dr. Ancker-Johnson follows:]

TESTIMONY

BEFORE

THE SENATE SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT
OF
THE COMMITTEE ON ENERGY AND NATURAL RESOURCES

April 7, 1987

Washington, D.C.

Dr. Betsy Ancker-Johnson
Vice President
Environmental Activities Staff
General Motors Corporation

Mr. Chairman and Members of the Committee, I am Dr. Betsy Ancker-Johnson, Vice President of General Motors Corporation in charge of the Environmental Activities Staff. I chaired the Physics Review Panel of the Energy Research Advisory Board, which just completed its work on the National Research Council Report, "Physics Through the 1990's".

Physics research is vital to the long-term development of advanced technology. One may convincingly argue that the United States still maintains a lead in many fields of physics and in much of the advanced technology development stemming therefrom, but the trend may well favor our competitors. It thus behooves the nation to consider very thoughtfully how much it spends on physics research, both fundamental and applied.

Physics, the science of the most fundamental aspects of nature, has had a profound impact on our view of the universe and on the manner in which we live our daily lives. Almost every aspect of modern society, including manufacturing, transportation, communications, and health care, has evolved directly or indirectly from the discovery and exploitation of fundamental physical phenomena. In particular, physics is involved in almost every aspect of research on the generation and efficient use and transport of energy. In recognition of the enormous importance of physics to the economic, material, and intellectual well-being of the country, the United States Government, largely through the Department of Energy, substantially supports many areas of physics.

The Department of Energy provides about $1.2 billion annually to support research in basic and applied physics, a sum that represents over half of the national total. In certain subfields -- Elementary-Particle, Nuclear, and Plasma Physics, the so-called "big physics" fields-- it is by far the major supporter. In the so-called "small physics" fields -- Condensed Matter (CMP) and Atomic,Molecular & Optical (AMO) -- DOE's support is also indispensible. About 40% of the independent researchers in CMP are supported by DOE. Although the percentage of AMO physicists supported by DOE is only about one-third as large as for CMP, both these fields depend virtually exclusively on DOE for the Major Research Facilities so crucial to their work. Clearly, decisions made by DOE concerning the support of particular areas, programs, and facilities have a major impact on the direction of the national effort in this critically important scientific discipline.

In 1983 the National Research Council (NRC) initiated a comprehensive survey of the state of physics in the United States. The Physics Survey Committee was chaired by Dr. W.F. Brinkman of Sandia National Laboratories and involved nearly 100 distinguished scientists with expertise in the different areas of physics as well as staff in several federal agencies and scientific societies. In May 1986 the survey produced a series of eight reports entitled "Physics Through the 1990's," highlighting past development and research opportunities in the various sub-fields of physics, and addressing general issues concerning the present state of the discipline in the U.S. that emerged during the review.

In July 1984, the Secretary of Energy requested that the Energy Research Advisory Board (ERAB) undertake reviews of forthcoming NRC reports. Following the publication of "Physics Through the 1990's," Mr. John Schoettler, Chairman of ERAB, appointed a panel, chaired by me with Dr. Hirsh Cohen of IBM as vice-chairman, to identify the areas addressed in the report that relate to DOE's programs and objectives, and either endorse, modify, or reject the NRC recommendations in those areas, while keeping in mind realistic budget constraints.

My report today provides a summary of our evaluation of the NRC report and presents our ERAB panel recommendations for DOE action, in response to the Secretary's request. If you wish, Mr. Chairman, I could provide a copy for your subcommittee.

Conclusions and Recommendations

The NRC report "Physics Through the 1990's" presents an eloquent case for the importance of this discipline in the intellectual life and aspirations of the modern world and its material well-being. In particular, the vital role of physics in furthering the objectives of the Department of Energy is made apparent by the report. The objectives for civilian R&D in the Department have been summarized in a recent ERAB report:

- To provide a robust array of economically attractive supply and efficient end-use options to help satisfy the Nation's unpredictable energy future;
- To conduct research to identify and mitigate the impact of energy production and use on health, safety, and the environment;
- To advance fundamental scientific knowledge and education in the disciplines that it supports.

Our ERAB Panel believes that physics research has major impact on all three objectives. Many examples are documented in the sub-field reports which form part of our review.

While the NRC report highlights many impressive intellectual and practical achievements of U.S. physics in the past ten years, and identifies many exciting opportunities in the near future, it also finds certain problems and issues facing the discipline today. Our ERAB Panel gave careful consideration to these questions in formulating the following recommendations:

1. A major concern expressed in the NRC report, and strongly shared by our Panel, is the unsatisfactory condition of basic physics research in universities. We recommend that DOE provide new funds for research and instrumentation to a number of small academic research groups performing research of high quality.

2. Our Panel was asked, in its charge letter, to keep in mind the unique role that DOE has in funding and managing major physics facilities. It is clear, from information received by our Panel, that nearly all existing facilities are being operated at a level which falls far short of what is scientifically optimal. We recommend that appropriate funding be provided to assure an adequate level of operation of these facilities and to upgrade the research instrumentation.

3. Our Panel recognizes the great scientific importance of the Superconducting Super Collider (SSC), which will enable experimental exploration of the key unresolved issues in elementary-particle physics; we understand the virtually unanimous support for this facility in the high-energy physics community. At the same time, the magnitude of the project means that it cannot be undertaken without a multibillion dollar incremental commitment to basic science over the next decade. The Energy Research Advisory Board's policy on this question is stated in an earlier report, "Guidlines for DOE Long-Term Civilian Research and Development," to the effect that the SSC should be funded subject to the requirement that other R&D priorities within DOE not be preempted. Our Physics Panel supports this position, and recommends that funding for the construction of the SSC be considered in a national context which is larger than that of DOE alone and independent of the facilities recommended under 4 below.

4. Our Panel recommends construction of other new facilities that have been identified by the respective user communities as being of paramount importance. In order of proposed starting dates, these are:

a. Compact Ignition Tokamak (FY 1988). This facility, whose construction is enthusiastically supported by the ERAB Magnetic Fusion Panel, is designed to perform a Duterium-Tritium ignition physics test, study confinement and control of a burning tokamak plasma, and provide a database for design of an engineering test reactor; it represents a logical and cost-effective approach towards a commercial fusion reactor.

b. 6-7 GeV Advanced Photon Source (FY 1988). This facility will generate x-ray beams of unprecedented intensity for up to 200 simultaneous experiments; its scientific and technical applications include advanced materials, semiconductors, lithography, catalysis, geology, medical imaging, bioengineering, and structural studies of key biological systems such as viruses and proteins.

c. Relativistic Heavy-Ion Collider (FY 1989). This accelerator will produce very high energy densities in nuclei, sufficient to lead to the formation of a "quark-gluon plasma', a unique form of matter that has not existed since the first instants of the universe.

d. Advanced Neutron Source (FY 1991). This research reactor will provide the most intense beams of steady-state neutrons in the world and a wide array of cold, thermal, and hot neutron beams. It will enable the United States to take a leading position in the application of neutron techniques to colloid and polymer science, biology, chemistry, metallurgy, and ceramics, as well as to the physics of solids and fluids and to nuclear and fundamental physics.

5. Our Panel emphasizes that the financing of new facilities should not be allowed to undermine the basic research programs on which the achievements of the new facilities will depend. We are particularly concerned about the steep decline in the support of plasma and fusion physics over the past three years, and recommend that future budgets reflect the need to maintain a balanced program.

6. The NRC report raises concerns about the education and training of physicists for the nationally important programs of the future. We recommend that DOE address the potential imbalance between supply and demand by initiating fellowship programs at both pre-doctoral and post-doctoral levels.

Our Panel estimates that the cost of carrying out the above recommendations will require an increase of approximately 50% in real funding for basic physics research in DOE between FY1986 and FY1992. We firmly believe such an increase to be justified by the great importance of physics to the programs and objectives of DOE. In the provision of economically attractive supply and efficient end-use energy options, the mitigation of the health and environmental impacts of these technologies, and the advancement of fundamental scientific knowledge, the different sub-fields of physics are playing an increasingly vital role. The recent discovery of ceramic materials that remain superconducting at liquid nitrogen, and perhaps even higher temperatures is just one example of the great potential of new concepts being explored. Physics research has also profound commercial applications, for example, ongoing developments in micro-electronic devices and computer components may ensure our Nation's economic viability in the 21st century. Finally, although not explicitly included in the charge to our Panel, basic physics research is essential to the Nation's defense capabilities.

The returns to the nation from its investment in basic and applied physics research have been tremendous and the future potential is even greater. I urge the Members of the Committee to continue their strong support of this vitally important field. Your problem currently is to decide whether to fund the SSC. I hope you will but I urge you not to do so at the expense of the rest of physics research.

Review of the National Research Council Report:

Physics Through the 1990's

Prepared by the Physics Review Panel
February 1987

A Report of the Energy Research Advisory Board
to the U. S. Department of Energy
Washington, D.C. 20585

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MAY 1986

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ABSTRACT

The National Research Council's report, "Physics Through the 1990's", highlights past developments and research opportunities in the various subfields of physics, and addresses general issues concerning the present state of the discipline in the U.S. At the request of the Secretary of Energy, the Energy Research Advisory Board has reviewed this report and identified the areas that relate to the programs and objectives of the Department of Energy. The present report provides the conclusions of this review and the recommendations for the Department's actions:

1. A major concern expressed in the NRC report, and strongly shared by the ERAB Panel, is the unsatisfactory condition of basic physics research in universities. The Panel recommends that DOE provide new funds for research and instrumentation to a number of small academic research groups performing research of high quality.

2. The Panel was asked, in its charge letter, to keep in mind the unique role that DOE has in funding and managing major physics facilities. It is clear, from information received by the Panel, that nearly all existing facilities are being operated at a level which falls far short of what is scientifically optimal. The Panel recommends that appropriate funding be provided to assure an adequate level of operation of these facilities and to upgrade the research instrumentation.

3. The Panel recognizes the great scientific importance of the Superconducting Super Collider and the virtually unanimous support for it in the high-energy physics community. At the same time, the magnitude of the project means that it cannot be undertaken without a multibillion dollar incremental commitment to basic science over the next decade. In line with previous ERAB policy statements on the SSC, the Panel recommends that funding for its construction be considered in a national context which is larger than that of DOE alone and independent of the facilities recommended under 4 below.

4. The Panel recommends construction of other new facilities that have been identified by the respective user communities as being of paramount importance. In order of proposed starting dates, these are:

   a. Compact Ignition Tokamak (FY 1988);
   b. 6-7 GeV Advanced Photon Source (FY 1988)
   c. Relativistic Heavy-Ion Collider (FY 1989)
   d. Advanced Neutron Source (FY 1991).

5. The Panel emphasizes that the financing of new facilities should not be allowed to undermine the basic research programs on which the achievements of the new facilities will depend. The Panel is particularly concerned about the steep decline in the support of plasma and fusion physics over the past three years, and recommends that future budgets reflect the need to maintain a balanced program.

6. The NRC report raises concerns about the education and training of physicists for the nationally important programs of the future. The Panel recommends that DOE address the potential imbalance between supply and demand by initiating fellowship programs at both predoctoral and postdoctoral levels.

The Panel estimates that the cost of carrying out these recommendations will require an increase in real funding for basic physics of approximately 50% over the next six years. It considers such an increase to be justified by the great importance of physics to the program and objectives of DOE. The Panel has carefully considered the impact of a no-growth budget over this period and concluded that such a budget would be devastating to DOE's mission. Should such circumstances occur, the Panel recommends that provision be found within the base budget to accomplish the recommendation relating to research support and instrumentation for small groups at universities.

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I. EXECUTIVE SUMMARY

Physics, the science of the most fundamental aspects of nature, has had a profound impact on our view of the universe and on the manner in which we live our daily lives. Almost every aspect of modern society, including manufacturing, transportation, communications, and health care, has evolved directly or indirectly from the discovery and exploitation of fundamental physical phenomena. In particular, physics is involved in almost every aspect of research on the generation and efficient use and transport of energy.

In recognition of the enormous importance of physics to the economic, material, and intellectual well-being of the United States, the Department of Energy supports substantial programs in many areas of physics. It provides over half of the federal funding for both basic and applied physics, and in certain subfields -- Elementary-Particle, Nuclear, and Plasma Physics -- it is by far the major supporter. Clearly, decisions made by DOE concerning the support of particular areas, programs, and facilities have a major impact on the direction of the national effort in this critically important scientific discipline.

In 1983 the National Research Council (NRC) initiated a comprehensive survey of the state of physics in the United States. The Physics Survey Committee was chaired by Dr. W.F. Brinkman of Sandia National Laboratories and involved nearly 100 distinguished scientists with expertise in the different areas of physics as well as staff in several federal agencies and scientific societies. In May 1986 the survey produced a series of eight reports entitled "Physics Through the 1990's," highlighting past development and research opportunities in the various subfields of physics, and addressing general issues concerning the present state of the discipline in the U.S. that emerged during the review.

In July 1984, the Secretary of Energy requested that the Energy Research Advisory Board (ERAB) undertake ad hoc Reviews of upcoming NRC reports, including the one described above. Following the publication of "Physics Through the 1990's," Mr. John Schoettler, Chairman of ERAB, appointed a panel, with Dr. Betsy Ancker-Johnson as chairman and Dr. Hirsh Cohen as vice-chairman, to review the NRC report, identify the areas addressed in that report that relate to DOE's programs and objectives, and either endorse, modify, or reject the NRC recommendations in those areas.

The present report provides the conclusions of the ERAB panel and their recommendations for the Department's action in response to the Secretary's request.

Conclusions and Recommendations

The NRC report "Physics Through the 1990's" presents an eloquent case for the importance of this discipline in the intellectual life and aspirations of the modern world and its material well-being. In particular, the vital role of physics in furthering the objectives of the Department of Energy can be readily appreciated by reading the report. The objectives for civilian R&D in the Department have been summarized in a recent ERAB report:

- To provide a robust array of economically attractive supply and efficient end-use options to help satisfy the Nation's unpredictable energy future;
- To conduct research to identify and mitigate the impact of energy production and use on health, safety, and the environment;
- To advance fundamental scientific knowledge and education in the disciplines that it supports.

The ERAB Panel believes that physics research has major impact on all three objectives. Many examples are documented in the subfield reports which form Section V of the present review.

While the NRC report highlights many impressive intellectual and practical achievements of U.S. physics in the past ten years, and identifies many exciting opportunities in the near future, it also finds certain problems and issues facing the discipline today. The ERAB Panel gave careful consideration to these questions in formulating the following recommendations:

1. A major concern expressed in the NRC report, and strongly shared by the ERAB Panel, is the unsatisfactory condition of basic physics research in universities. The Panel recommends that DOE provide new funds for research and instrumentation to a number of small academic research groups performing research of high quality.

2. The Panel was asked, in its charge letter, to keep in mind the unique role that DOE has in funding and managing major physics facilities. It is clear, from information received by the Panel, that nearly all existing facilities are being operated at a level which falls far short of what is scientifically optimal. The Panel recommends that appropriate funding be provided to assure an adequate level of operation of these facilities and to upgrade the research instrumentation.

3. The Panel recognizes the great scientific importance of the Superconducting Super Collider (SSC) and the virtually unanimous support for it in the high-energy physics community. At the same time, the magnitude of the project means that it cannot be undertaken without a multibillion dollar incremental commitment to basic science over the next decade. In line with previous ERAB policy statements on the SSC, the Panel recommends that funding for its construction be considered in a national context which is larger than that of DOE alone and independent of the facilities recommended under 4 below.

4. The Panel recommends construction of other new facilities that have been identified by the respective user communities as being of paramount importance. In order of proposed starting dates, these are:

a. Compact Ignition Tokamak (FY 1988)
b. 6-7 GeV Advanced Photon Source (FY 1988);
c. Relativistic Heavy-Ion Collider (FY 1989);
d. Advanced Neutron Source (FY 1991).

5. The Panel emphasizes that the financing of new facilities should not be allowed to undermine the basic research programs on which the achievements of the new facilities will depend. The Panel is particularly concerned about the steep decline in the support of plasma and fusion physics over the past three years, and recommends that future budgets reflect the need to maintain a balanced program.

6. The NRC report raises concerns about the education and training of physicists for the nationally important programs of the future. The Panel recommends that DOE address the potential imbalance between supply and demand by initiating fellowship programs at both predoctoral and postdoctoral levels.

The Panel estimates that the cost of carrying out the above recommendations will require an increase of approximately 50% in real funding for basic physics research in DOE between FY1986 and FY1992. It considers such an increase to be justified by the great importance of physics to the programs and objectives of DOE.

The Panel has carefully considered the impact of a no-growth budget over this period. The primary conclusion is such a budget would be devastating to the mission of the Department of Energy. Should such circumstances occur, the Panel believes that construction of facilities whose funding has already been initiated, including CEBAF and the 1-2 GeV Advanced Light Source, should be continued, and a start made on the Compact Ignition Tokamak and the 6-7 GeV Advanced Photon Source. If, during the next five years, even this budget level cannot be sustained, the Panel recommends that provision still be made within the base budget to accomplish recommendation 1 above relating to research support and instrumentation for small groups at universities.

II. BACKGROUND AND CHARGES

Physics, the science of the most fundamental aspects of nature, has had a profound impact on our view of the universe and on the manner in which we live our daily lives. Almost every aspect of modern society, including manufacturing, transportation, communications, medicine and health care, has evolved directly or indirectly from the discovery and exploitation of fundamental physical phenomena. In particular, physics is involved in almost every aspect of research on the generation and efficient use and transport of energy.

In recognition of the enormous importance of physics to the economic, material, and intellectual well-being of the United States, the Department of Energy supports substantial programs in many areas of physics. According to a recent survey (1) DOE supported in 1984 59% of the national basic research effort in physics and 51% of the applied research effort; in certain areas DOE is in fact the major supporter -- 90% in Elementary-Particle Physics, 81% in Nuclear Physics, and 96% in Plasma Physics. Clearly, decisions made by DOE concerning the support of particular areas, programs and facilities have a major impact on the direction of the national effort in this critically important scientific discipline.

In 1983 the National Research Council initiated a comprehensive survey of the state of physics in the United States. With the help of nearly 100 distinguished scientists in its Physics Survey Committee and panels, and staff in several federal agencies and scientific societies, the survey produced in May 1986 a series of eight reports entitled "Physics Through the 1990's." Seven of these highlighted past developments and research opportunities in major subfields of physics:

Atomic, Molecular and Optical Physics
Condensed-Matter Physics
Elementary-Particle Physics
Gravitation, Cosmology and Cosmic-Ray Physics
Nuclear Physics
Plasmas and Fluids
Scientific Interfaces and Technological Applications

In the words of the Chairman of the Physics Survey Committee, Dr. W.F. Brinkman, "these volumes document a physics enterprise that is vital, creative, and productive."

An eighth volume, entitled "An Overview", addresses important general questions that emerged during the review, including identification of the areas of physics that have shown most progress in the past decade, problems of educating the next generation of physicists, the future production of scientifically and technologically trained manpower, the U.S. position in the world physics community, and the question as to whether excellence is truly supported in the present funding system.

In July 1984, the Secretary of Energy requested that the Energy Research Advisory Board (ERAB) undertake ad hoc Reviews of upcoming NRC reports including the one described above. He recognized that the recommendations and information in the reports could have major effects on the Department's science efforts in programs of the Office of Energy Research and also on those in other components of the Department. He requested that the Review:

- o Examine the substantive recommendations of the reports,
- o Assess the significance in terms of the Department's programs and responsibilities,
- o Recommend appropriate action and,
- o Complete the assessment in six months.

Subsequently, in May 1986, Mr. John Schoettler, Chairman of ERAB, appointed a panel, with Dr. Betsy Ancker-Johnson as chairman and Dr. Hirsh Cohen as vice-chairman, to review the NRC report. Mr. Schoettler's letter asked the Panel to concentrate on the areas addressed in the NRC report that correspond to the DOE's programs and either endorse, modify, or reject the NRC recommendations in those areas. The Panel was asked to keep in mind the unique role that the Department has in funding and managing most of the major facilities on which several areas of physics research critically depend.

Mr. Schoettler's letter stated that to conclude that all the NRC recommendations must be supported without prioritization would be of little use to the Department, and the Panel was asked to keep in mind the budget constraints under which DOE is operating now and in the future. As a framework for prioritizing the recommendations, the Panel was reminded of the objectives for civilian R&D in DOE, summarized in a recent ERAB report:(2)

- o To provide a robust array of economically attractive supply and efficient end-use options to help satisfy the Nation's unpredictable energy future;

- o To conduct research to identify and mitigate the impact of energy production and use on health, safety and environment;

- o To advance fundamental scientific knowledge and education in the disciplines that it supports.

The panel consisted of eight ERAB members and four external members chosen to augment the expertise coverage of the physics subfields. The Panel as a whole thus represented the different subfields of physics and several other scientific fields with close interfaces with physics. It included scientists from universities, corporations and national laboratories and represented many geographical regions of the United States.

III. REVIEW PROCEDURES AND CONSIDERATIONS

The Panel held four publicly announced meetings: the first was held in Washington, D.C. on June 13, 1986, the second in New York on August 25, 1986, and the third and fourth in Chicago on October 7, 1986 and January 14, 1987. Appendix A contains the agenda of these meetings.

In addition to the NRC Physics Review, several topical reviews pertinent to various subfields of physics have been carried out in the last few years, especially by ERAB, NRC, and the White House Science Council. These reports were made available to the members of the Panel. They are listed in the bibliography in Section VI.

The first meeting was devoted to testimony from Dr. A. Trivelpiece, Director of the Office of Energy Research (OER), DOE, the Associate Directors of OER, and representatives of other agencies providing major support for physics research. The Panel also had an extended discussion with Dr. W.L. Brinkman, Chairman of the NRC Physics Survey Committee. At the end of this meeting, subpanels were appointed to review the major subfields of physics, along the categories used in the NRC Review. The subpanels and their membership are listed below:

Atomic, Molecular, and Optical Physics (N.F. Ramsey, J.R. Huizenga)
Condensed Matter Physics (D. Chang, H.G. Cohen)
Elementary Particles, Gravitation, Cosmology and
  Cosmic Ray Physics (J.D. Bjorken, R.G. Noll)
Nuclear Physics (J.P. Schiffer, R.H. Pry)
Plasma and Fluid Physics (N. Krall, A. Hansen)
Scientific Interfaces and Technological Applications
  (M. Dresselhaus, D. Chang, R.G. Noll)

Drafts of the subfield reports in Section V were prepared by these subpanels. The subsequent meetings were devoted to presentations by these subpanels and discussion of general issues and priorities. At the second meeting, it became clear that many of the crucial questions of funding priorities revolved around the operation and construction of major facilities, including, but not limited to, the proposed Superconducting Super Collider. Information about both existing and proposed facilities was, therefore, requested from DOE; this is summarized in Appendix B. A summary of current levels of overall DOE support for physics is given in Appendix C.

IV. CONCLUSIONS AND RECOMMENDATIONS

"Physics Through the 1990's" presents an eloquent case for the importance of this field in the intellectual life and aspirations of the modern world and its material well-being. To appreciate the excitement of the work at the frontiers of the various subfields of physics, the reader is urged to read through the seven subfield volumes of the NRC report. It is impossible to do justice to the ideas and information presented in these volumes within the space of the present review.

The importance of physics to the objectives of the Department of Energy can also be readily appreciated by reading the NRC report. To take the first of the objectives listed in the charge letter -- the provision of economically attractive supply and efficient end-use energy options -- the need for basic physics research in order to improve efficiency, and in some cases even to establish commercial feasibility, is apparent in nearly every energy technology. For example, major improvements in the efficiency of the combustion of hydrocarbon fuels have resulted from the improved understanding of the combustion process obtained with spectroscopic techniques. New types of catalysts have been developed as a result of the tremendous advances in surface physics in the last ten years. The discovery of the remarkable electrical properties of certain amorphous semiconductors has led to a new concept for low-cost photovoltaic cells. Advances in understanding the behavior of ionized plasmas in magnetic confinement, made possible by the diagnostic techniques of atomic, molecular, and optical physics, in addition to the emerging maturity of basic plasma physics, have moved magnetic fusion closer to its goal. And especially important over the whole range of energy technologies is the improved performance of materials under extreme environments such as high temperature, high pressure, corrosive gases, or strong radiation fields, or when optimized electrical, magnetic, or mechanical properties are required.

In connection with mitigating the health and environmental impacts of energy production and use, spectroscopic techniques, especially those involving lasers, have proved invaluable for detecting the release of hazardous agents and monitoring subtle changes in the environment. Modern sensing and control equipment is largely based on solid-state devices.

The advancement of fundamental scientific knowledge among the disciplines it supports is, of course, the objective most broadly furthered by basic physics research. In fact, DOE has the prime responsibility in three subfields: elementary-particle, nuclear, and plasma physics, a mandate inherited from the former Atomic Energy Commission. In this connection it is important to emphasize the strong and complex interconnections between physics and other branches of science, which have been eloqently documented in the "Scientific Interfaces and Technological Applications" volume of the NRC report. These interfaces connect physics not only to the disciplines which have been traditionally assocated with it, such as chemistry, electronics, metallurgy, ceramics and polymer science, but also to ones further afield like biology, medicine, geology, and oceanography, where techniques discovered during the course of fundamental research in physics are being put to uses which could not have been imagined at the time of their discovery. Much of the physics research supported by DOE is directed towards the discovery and development of techniques which may have broad application in other fields.

These include "bench-top" devices as well as the major user facilities, many of which are based on accelerator concepts developed under the High-Energy Physics program and which are being extensively used by scientists from different subfields of physics and a broad range of other disciplines. This point is addressed below in the discussion of major user facilities.

The commercial implications of much of the physics work supported by DOE, in both its research programs and the major user facilities, should also be emphasized: ongoing developments in microelectronic devices, new computer memories and even new concepts in computer systems themselves, may ensure the nation's economic viability in the 21st century. The NRC volume on Scientific Interfaces and Technological Applications expresses broad concern over the ability of the United States to rapidly convert its wealth of discoveries and increased understanding in physics into products and services for social and economic betterment. This concern for a more rapid utilization of science for social benefit, for both international competitiveness and internal programs, is receiving increased attention. DOE is addressing this concern in a number of recent initiatives and would be well advised to review these initiatives using a broad, systematic approach.

Finally, although not specifically contained in the charge for this review, the importance of basic physics research to the defense activities of DOE as well as the Department of Defense are obvious, in that devices for guidance, surveillance, detection, and communication, able to function under conditions of high radiation and electromagnetic fields, are crucially dependent on advanced understanding of the physical processes involved.

The NRC report highlights many impressive intellectual and practical achievements of U.S. physics research in the past ten years, and identifies many exciting opportunities in the near future. At the same time it identifies certain problems and issues facing the discipline today. For example, it is clear that the leadership by the United States in most areas of physics is not as dominant as it was 10 or 20 years ago. The report cites evidence that the fraction of the U.S. GNP invested in basic research in physics is now a quarter to a half of that in some other high-technology societies. It regards the current investment in basic research as grossly inadequate. **Basic rectification of this situation requires an approximate doubling of the U.S. investment.** In view of the importance of physics to the objectives of DOE, the decisions to be made concerning the opportunities, and problems and issues identified in the NRC report will, without doubt, have a major impact on these objectives.

The remainder of this section describes the conclusions of the Panel concerning the general issues raised in the NRC report as they affect DOE, and the actions which the Panel recommends to DOE. The subsequent section, Section V, gives detailed recommendations for the individual subfields.

A. Basic Research in Universities

A major concern expressed in the NRC report, and strongly shared by this Panel, is the perilous condition of basic research in universities. University research groups consist usually of a single professor with a few graduate students and, if fortunate, a postdoctoral appointee; they work with equipment in their own laboratories supplemented, perhaps, by periodic visits to centralized user facilities. They are responsible for training the majority of Ph.D. students in physics, another area of concern, as discussed below. (Appendix D gives statistical information on Ph.D. production [illegible] subfield). The largest subfields of small-group research are atomic, molecular, and optical physics and condensed-matter physics, two subfields which make large contributions to commercial applications and to technology, especially the energy technologies, as is apparent from the discussion above. Yet support for small-group physics at universities has been decreasing. Hiring has been cut back, with the consequent aging of university faculties. Even in fields where a number of academic positions have opened up, such as experimental condensed-matter physics, it is hard to attract qualified candidates, who often choose other pursuits over the hard struggle to obtain an adequate level of research support from federal agencies.

In conjunction with the above, there is special concern about the inadequate state of instrumentation in U.S. physics laboratories. Many studies in the past five years have reported a lack of essential equipment in many university laboratories. While the problem ranges from the shortage of large equipment in the million-dollar range to small instruments such as oscilloscopes, it is especially severe in small university groups, whose average grant sizes of the order of $100,000 per year are simply inadequate to satisfy minimal equipment needs.

In the light of the above considerations, the Panel's first recommendation is that DOE provide new funds for research and instrumentation to a number of small academic research groups performing research of high quality. The recommended funding increases are given in the atomic, molecular, and optical and condensed-matter physics subfield reports. In the case of larger instruments, for example advanced Nuclear Magnetic Resonance installations or Molecular Beam Epitaxy equipment, it may be appropriate to set up local centers where the instrumentation is used jointly by a number of groups. Appropriate procedures would have to be set up for operation and use of equipment in such a mode.

B. Major Facilities

1. General Considerations

Issues relating to major facilities loom large in the NRC report, and occupied a significant amount of attention of the present review. In elementary-particle, nuclear, and plasma physics, the paramount importance of large, state-of-the-art installations is self-evident. But also in fields which traditionally have had a "small-science" flavor, such as atomic, molecular, optical, and especially condensed-matter physics, centralized user facilities are playing an increasingly important role, and the allocation of resources among these facilities and campus- or home-laboratory-based equipment is the subject of a continuing dialogue in these communities. In this context it is important to keep in mind that in both cases the actual research may be performed by small groups, frequently based at universities. In the case of condensed-matter physics, the subject of major materials facilities has been recently addressed in NRC(3) and ERAB(4) reviews which are parallel to those which form the subject of the present report. The major physics-related facilities, existing and proposed, identified by the relevant program offices in DOE are listed in Appendix B.

It is clear from the information received from DOE, and also from testimony received during the Panel's first meeting, that nearly all existing facilities are operated at a level which falls far short of what is scientifically optimal. Many facilities run for 20 to 30 weeks per year, far short of the 40 to 50 weeks per year which would be considered scientifically optimal in most cases. At least in the case of the fusion program, recently completed facilities are simply not operated at all because of lack of funds. This situation, which extends over all the physics subfields involving major facilities, clearly represents an inefficient use of the nation's resources. The Panel recommends that appropriate funding be provided to assure an adequate level of operation of these facilities and to upgrade the research instrumentation at the facilities to improve their effectiveness. Along with this commitment comes an obligation to phase out older facilities which are no longer on the scientific frontier. The Panel did not feel qualified to make recommendations concerning specific facilities to be closed down, but recommends that these decisions be made by the appropriate program offices in consultation with the relevant scientific community.

2. The Superconducting Super Collider

Decisions on the construction of new facilities present the most difficult questions, in view of the large budgets involved and because these decisions are clearly going to shape U.S. science for the rest of this century. Dominating all new construction in DOE is the proposed Superconducting Super Collider (SSC). The NRC report makes a compelling argument for this machine, which would provide the very high energies that are very widely believed to be necessary for exploring key unresolved issues in elementary-particle physics. This Panel recognizes the great scientific importance of the SSC and the virtually unanimous support for it in the high-energy physics community. At the same time, the magnitude of this project, over three billion dollars or three times the annual DOE physics budget, means that it cannot be undertaken without a multibillion dollar incremental commitment to basic science over the next decade. The Panel recommends that funding for SSC construction be considered in a national context which is larger than that of DOE alone and independent of the four facilities recommended in 3 below. This recommendation is in line with previous ERAB policy statements on the SSC, which recommended funding under certain conditions, including the requirement that other R & D priorities within DOE not be preempted(2).

3. Other New Facilities

Recommendations for other new facilities are discussed in the subfield reports. According to cost estimates received from DOE program offices, it appears that the following four new facilities can be accommodated within the budget discussed below (Section D). In assessing the funding recommended for facilities in comparison with that for research, it is important to keep in mind the multipurpose use of the facilities and the interdisciplinary nature of much of the research that is carried out at them. For example, the materials research facilities are heavily used by chemists, biologists, and polymer scientists, among others, as well as condensed-matter physicists. In light of these considerations, the Panel subscribes to the recommendations for new facilities contained in the subfield reports. The recommended new facilities (in order of proposed starting dates) are:

a. Compact Ignition Tokamak (FY1988);
b. 6-7 GeV Advanced Photon Source (FY1988);
c. Relativistic Heavy-Ion Collider (FY1989);
d. Advanced Neutron Source (FY1991).

The dates in parentheses represent the dates proposed by the Laboratories for starting construction. It should be recognized that these facilities represent careful and deliberate choices on the part of the respective user communities out of a much longer list of possible options.

The Panel emphasizes that the financing of new facilities should not be allowed to undermine the basic research programs on which the achievements of the new facilities will depend. The Panel is particularly concerned about the steep decline in the support of plasma and fusion physics over the past three years and recommends that future budgets reflect the need to maintain a balanced program.

C. Education and Manpower

The NRC report raises concern about the education and training of physicists for the nationally important programs of the future. As the largest government supporter of physics research, DOE has a special responsibility in this area. In particular, the defense programs of DOE will have a continuing need for U.S. citizens with good physics training. At present, a precarious balance exists between supply and demand, maintained in part by a large enrollment of graduate students from abroad. Starting in the 1990's, however, increasing retirement rates in university departments and higher demands for physicists in applied research and technology will likely lead to a serious undersupply of physics graduates. The Panel recommends that DOE address the potential imbalance between supply and demand by initiating fellowship programs at both predoctoral and postdoctoral levels. Predoctoral fellowships, providing support for graduate work in physics which is not tied to a specific research effort or to teaching responsibilities, will make it more attractive for scientifically talented American students to choose graduate careers in physics and thereby raise the level of quality among physics graduate students. Postdoctoral fellowships will provide more opportunities for recent Ph.D.'s to continue research in their area of training and reduce the number of new Ph.D.'s (presently about one-third) who now abandon basic research after completing their graduate studies.

D. Budgetary Impact

The Panel estimates that the total cost of the recommendations listed above (excluding SSC construction) will require an increase in real dollars of approximately 50% in DOE funding for basic physics research between FY 1986 and FY 1992. This increase amounts to an average of 7% per year for the six years. It would not be equal among all subfields, however. The Panel considers such an increase to be justified by the great importance of physics to the programs and objectives of DOE, as summarized above and described in more detail in the subsequent sections.

The Panel has also carefully considered the impact of a no-growth budget over this period. The primary conclusion is that such a budget would be devastating to the mission of the Department of Energy. Should such circumstances occur, the Panel recommends that, in spite of the financial difficulties incurred, provision be found within the base budget to accomplish the recommendation discussed above relating to research support and instrumentation for small groups at universities. It is also essential to continue, although at a less than ideal pace, with the construction of facilities already funded, including CEBAF and the 1-2 GeV Advanced Light Source, and to make a start within the next five years on the Compact Ignition Tokamak and the 6-7 GeV Advanced Photon Source. Clearly, these initiatives can be undertaken only at great cost to ongoing and proposed new research programs, involving the termination of a significant number of productive research groups, continued delays in the establishment of new groups around promising young scientists, and the premature closure of older but highly productive major facilities. A level budget would also imply that the SSC would not be built, or at least would be substantially delayed, with a crippling effect on U.S. leadership in Elementary-Particle Physics.

E. International Aspects

Like all sciences, physics is international by nature: developments in one country are normally freely available to those in others, although this is coupled with competition among scientists and among countries, competition that has a healthy and stimulating effect. Also, to an increasing degree, cooperative efforts among different countries are helping to mitigate the escalating expenses involved in large research projects. Nevertheless, the nation most instrumental in developing new knowledge and new technology, and the nation with the most skilled and technologically versatile and sophisticated manpower, is also in the best position to utilize new knowledge. Although the long-term technological and economic effects of a strong basic research effort are difficult to assess precisely, such effects have strengthened the U.S. enormously in the past and are certain to be even more important in the future. Maintaining and strengthening the U.S. international position in basic physics research should be an important national goal and one that the Department of Energy can influence in a major way.

V. SUBFIELD REPORTS

This section contains the detailed recommendations for the five subfields that were addressed by the Panel. Since relatively little work on Gravitation, Cosmology, and Cosmic-Ray Physics falls within DOE's purview, this subfield was combined with Elementary Particle Physics for the purposes of the present study. Also, the Scientific Interfaces and Technological Applications volume of the NRC report is not addressed explicitly: some recommendations relating to these areas have, however, been made in Section IV of this report.

Each subpanel was asked to (1a) identify the NRC Report's recommendations in its subfield that were applicable to DOE and (1b) recommend the ERAB Panel's response to these. It was asked to comment on (2) the importance of the subfield for the R&D objectives of DOE, (3) other impacts on the nation's science and technology, (4) the financial consequences of the Panel's recommendations, and (5) the impact of a level budget for the next three years. The Panel endorses the recommendations expressed in each subfield report (subject to the conditions expressed in Section IV concerning the SSC).

A. Atomic, Molecular and Optical Physics

1. Recommendations Applicable to DOE

a. Recommendations of the NRC Report

The goals of atomic, molecular and optical physics (AMO physics) are to elucidate the fundamental laws of physics, to understand the structure of matter and how matter evolves at the atomic and molecular level, to understand light in all its manifestations, and to create new techniques and devices. The Panel on AMO Physics of the NRC Physics Survey Committee reviewed the spectacular advances in this field that have occurred during the past decade and the extensive applications of the discoveries in this field to other research areas and to the establishment of major new industries.

Despite these scientific advances and extensive practical applications during the past decade, the government financial support of research in the AMO field has fallen disastrously in recent years; between 1972 and 1983 the funding in constant dollars fell by 30%, despite the markedly increased intrinsic costs of research due to the extensive use of high-powered lasers and other costly instruments. In order to compensate for more than a decade of underfunding, to provide badly needed new instruments, and provide opportunities for young scientists, the NRC AMO Panel recommends a four-year program in which the annual federal support of AMO research is increased substantially. One suggested scenario is to increase the funding for base support of 35 groups per year by $200,000 per group for each of the next four years, leading to 140 groups with enhanced support at the end of the four-year period. At the end of that period, base support of AMO research by the combined efforts of all funding agencies will have increased by $28 million (equivalent to the overall funding of basic Atomic, Molecular and Optical physics by the Federal Government in universities and allied institutes in 1983). Special one-time support is recommended also to re-equip AMO laboratories. Again, a possible scenario is to re-equip 35 groups per year at a cost of $11 million per year for each of the next four years.

The NRC AMO Panel additionally stated that three non-major* accelerator based atomic physics facilities are needed for the U.S. to seize a leadership role in this field.

The NRC AMO Panel recommends that primary (but not overriding) priority for the added support be given to the following eight areas:

(i) In Atomic Physics

1) Fundamental Tests and High Precision Techniques
2) The Many-Electron Atom
3) Transient States of Atomic Systems

(ii) Molecular Physics

1) The Physics of Isolated Molecules
2) The Physics of Molecular Collisions

(iii) Optical Physics

1) New Light Sources
2) Advanced Spectroscopy
3) Quantum Optics

Although AMO Physics has extensive applications to the problems of the DOE, the total amount of DOE funding of AMO research in physics is quite small, coming to only $6.9 million in Fiscal 1984 out of the total federal AMO research support of $44.9 million. It should be noted, however, that AMO science is also supported by the DOE in other research disciplines such as physical chemistry. The total DOE support of AMO science from the Basic Energy Science budget in fiscal 1985 was approximately $20.8 million, not including major facilities. This budget included $7.0 million for physics. In addition, the DOE Military Applications budget supports projects such as the free-electron laser.

The AMO panel is not explicit in its recommendation to each specific government agency, although it is emphatic in its recommendations for major increases.

b. Recommendations by the ERAB Panel

The ERAB sub-panel on AMO Physics fully agrees with the recommendations of the NRC Panel as to the importance and viability of AMO physics. It also agrees as to the general areas to be given priority. Since the increased individual sums to go to the different areas are relatively small, there is no advantage to specifying relative priorities for each of the individual areas since significant increases should be spread over most or all of the priority areas.

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*A major facility is defined, in the present review, as one whose annual operating budget exceeds $5 million.

Since the NRC panel does not make explicit recommendations for funding increases by individual government agency, the ERAB AMO panel makes the following explicit recommendation for increased DOE support of AMO research:

(i) The fiscal 1987 DOE anticipated budget of $7.7 million for AMO physics should be increased by $2.0 million per year (includes operating and equipment costs) for each of the next four years to bring the total DOE budget for AMO physics to $15.5 million per year by the end of the four-year period.

(ii) In selecting individual projects for increased support, priority should be given to the following:

1) The quality and promise of the proposal.
2) The above eight general areas of AMO research recommended by the NRC.
3) Increasing the support of the most effective AMO groups to permit them to pursue effectively new scientific opportunities and to modernize their instrumentation.
4) Increasing the opportunities for new young investigators to enter the field and to establish new groups.
5) The potential applicability and value of the project in contributing to the energy program of the United States.

2. Importance for the R&D Objectives of DOE

a. Various spectroscopy, laser, and atomic recombination techniques developed in AMO physics are of great value in studying most energy production methods, including especially chemical combustion and fusion.

b. AMO techniques, including spectroscopy and lasers, have been among the most valuable means for monitoring the release of chemical products from combustion that are harmful to the environment.

c. The field of AMO physics has been particularly effective in advancing fundamental scientific knowledge at relatively low cost. The field of quantum electrodynamics mostly originated from experiments in AMO physics, and critical tests of quantum electrodynamics and of fundamental symmetries in nature are currently in progress. Atomic effects of the weak interaction, including those of neutral weak currents, are now being accurately studied in AMO physics.

d. The field of AMO physics has been particularly effective in supplying scientific manpower at relatively low cost. The most effective research groups are ordinarily sufficiently small in size to be based at universities. According to the NRC Overview (pages 98 and 128) the field of AMO physics in 1983 produced 19% of the physics Ph.D.'s but in so doing the field consumed only 3.2 % of the federal operating budget for physics. The cost effectiveness of the AMO field in training Ph.D.'s would be even greater if construction costs were included.

3. Other Impacts

a. The AMO field has made highly cost-effective contributions to the national technological base. The laser instrumentation and medical equipment industries are all heavily dependent on the AMO developments, and AMO techniques and devices are used in many industries.

b. The AMO field has had tremendous impact on U.S. defense capabilities. Lasers and masers were both invented in the field of AMO physics. The laser is central to many offensive and defensive weapons in current practical use as well as for alternatives in the Strategic Defense Initiative. Modern optical devices are extensively used by the military, and atomic clocks are used for navigation and for other military applications.

c. Past U.S. discoveries in AMO physics including lasers, masers, quantum electrodynamics, non-linear optics, NMR, etc. have contributed greatly to past U.S. scientific leadership. As Japan and Europe have markedly increased their AMO research expenditures in recent years, the U.S. position of leadership is eroding.

d. The field of AMO physics has been particularly effective in contributing to other fields. NMR, for example, developed from this field, has been extensively used as a basic tool in chemistry and biology; and now, in medicine, NMR gives promise of becoming one of the most effective tools in medical diagnosis. Likewise, lasers are already used extensively in almost all scientific disciplines. Molecular beam techniques are widely used to study chemical reactions in detail. Optical devices may lead to much more powerful future computers. Many other examples are provided in the NRC AMO report.

4. Financial Consequences of the ERAB Recommendations

The financial consequences of the recommended AMO program are relatively minor. The costs to the DOE are low in contrast to the benefits derived. The recommendation calls for an increase in the DOE annual budget for AMO research of $2.0 million per year (includes operating and equipment costs) for each of the next four years. The financial benefits to the country should be large, since the AMO field has been particularly fruitful at spawning new industries. Although no termination of AMO research can be anticipated at the present time, the DOE progress in this field should be periodically reviewed to consider possible reduction or elimination of activities within it.

The relative need for instrumentation and for operating costs varies so markedly from one experiment to another in the AMO field that it is unwise in an ERAB report to recommend a specific distribution between the two. Instead, the relevant DOE staff in their program reviews should take seriously the recommendations of ERAB and of the NRC Committee on the great need for increased instrumentation.

The DOE should also review the distribution of research funds between universities and national laboratories, taking into account the NRC Committee's arguments for increasing the fraction of university research. However, a specific distribution of funds between universities and national laboratories is not recommended in the present report.

The primary recommendation is for an increase of $2.0 million per year for the four-year period 1987-1991. In the subsequent year an increase of 4% is somewhat arbitarily assumed, and this assumption should be reviewed in subsequent years. With these assumptions, the budgets for DOE AMO research in the Basic Energy Sciences for the period 1986-1992 (in millions of 1986 dollars) are 7.0, 7.7, 9.7, 11.7, 13.7, 15.7, and 16.3. All of the indicated funds are for operating and equipment allocations. No AMO construction funds are requested in these budgets.

5. Impact of a Level Budget for Three Years

Since the support of AMO research in constant dollars has markedly fallen during the past decade while the cost of major instruments such as high-power lasers has greatly increased, a level research budget in the AMO field for the next 3 years would have terrible consequences. Already the support of some excellent research groups in the field has been terminated, and most of the supported groups are grossly underfunded. A flat budget would lead to the termination of highly productive research groups, would prevent adequate support and instrumentation to continued research groups, and would make it extremely difficult for promising young scientists in the field to establish new research groups. It is, of course, not possible to predict what discoveries would be missed, and what new industries would be established in Japan and Europe instead of in the United States as a result of a level budget, but the probability of such U.S. failures would be high, since the Japanese and Europeans are well supported in the AMO fields.

If the budget for AMO research is held constant, the relevant DOE staff should review even more critically the DOE research in the AMO areas for possible program improvements, taking into account the above recommendations and those of the NRC Committee. However, it should be emphasized that such an exercise will not significantly diminish the impact of a level budget, since at present the fundamental problem for the support of research in the AMO field is not the distribution of research support within the field, but the inadequate total amount of support available.

B. Condensed Matter Physics

1. Recommendations Applicable to DOE

a. Recommendations of the NRC Report

Condensed Matter Physics (CMP) is the study of the physical properties of solids and liquids, and their interfaces. The NRC CMP Panel documents a host of exciting theoretical and experimental developments which have occurred in the field during the past decade. The progress that has been made in artificially structured materials, in the understanding of disordered systems, chaos, and phase transitions, and in the use of synchrotron radiation and high-resolution atomic probes, has prepared the way for continued fundamental and applied achievements in CMP in the next decade. In addition, the NRC panel clearly showed that CMP contributes directly and often immediately to many aspects of the nation's economy and security. It is a field in which the nation must maintain leadership. This will happen only if manpower and facilities are strengthened and the research itself adequately supported. The NRC CMP panel made two sets of recommendations to provide increased support, one for independent researchers and one for major national facilities.

Increased support of individual researchers is the Panel's highest priority. Based on 1984 funding levels, the Panel recommends:

i. an annual increase of at least 3-4% in the number of grants and an increase in the size of grants, for the next 4 years;

ii. support of instrumentation programs on a continuing basis, at a level of 20-25% of the present R&D budget; and

iii. support of computing needs on a continuing basis, at a level of 10% or more of the present R&D budget.

The recommendations are summarized in the NRC report by requesting a 20%/year increase in individual project research funding for three years.

For the increased support of national facilities, the panel recommended:

i. instrumentation of existing high performance reactor neutron sources (10 new instruments);

ii. enlargement of the Los Alamos Neutron Scattering Center instrument hall for the pulsed spallation neutron source, and continued interim operation of Argonne's Intense Pulsed Neutron Source;

iii. planning for an advanced neutron source (either pulsed or steady state);

iv. completion of the current new generation of synchrotron facilities;

v. insertion of undulators and wigglers on existing storage rings;

vi. planning for an all-insertion-device machine for hard x-rays (e.g., a 6 GeV synchrotron) with construction to be completed in the early 1990's;

vii. an improved high-magnetic-field facility;

viii. advanced instrumentation of electron microscope facilities;

ix. and facilitation of non-expert-user access to national facilities.

The NRC-CMP panel agreed with the recommendations of the Seitz-Eastman panel(3) and the ERAB panel(4) which reviewed the Seitz-Eastman report in emphasizing that the overall highest priority should be placed on the support of individual researchers and on the upgrading of existing national facilities as a prerequisite for the construction of needed major new facilities. In addition, it called for universities, government, and industry to promote cooperative research, personnel exchange, access, and in-house industrial research.

Because of DOE's extensive involvement in materials R&D, all but one of the recommendations apply directly to the Department. The one exception is the recommendation for an improved high-magnetic-field facility, for which the National Science Foundation has taken responsibility.

The cost of implementing the NRC recommendations is appreciable. For the support of independent researchers, the NRC CMP panel recommended an annual increase in R&D funding of 20% per year in constant dollars for 3 years. For the support of major facilities, the NRC recommendations resemble quite closely those made in the Seitz-Eastman report and in the ERAB review of the Seitz-Eastman report. Accordingly, the NRC recommendations for upgrading existing national facilities and constructing new facilities could be implemented with the ERAB (Seitz-Eastman) proposal of an increase in the Division of Materials Sciences (DMS) construction budget by 6-7 million constant dollars per year for a total of 8 years, and an increase in the DMS operating budget by 7-8 million constant dollars per year for the first 3 years. Based on the detailed estimates of construction costs generated by DOE this past year, this increased funding would be adequate to construct a 6-7 GeV advanced photon source as well as an advanced neutron source during the next decade, in addition to the 1-2 GeV light source for which construction funds starting in FY87 have been approved.

If 75% of the major materials facilities (MMF) operation and construction costs are attributed to CMP work, then direct implementation of the NRC recommendations would result in a greater than 20% annual increase in the DOE CMP budget for the first 3 years. These are large rates of increase, especially for DOE which already bears a large share of CMP support.

b. Recommendations by the ERAB Panel

As we discuss in Section 2, Condensed Matter Physics is important for energy discovery, distribution, control and use and has similar importance for environmental control. The nation's energy system has a dependency on the quality and the amount of physics done in this field. We therefore recommend that a growth program for CMP should be implemented in DOE.

The Department of Energy is the largest reported single source of materials R&D support in the United States. DOE's materials R&D budget was close to $350 million annually in 1985 and is 1/3 of the total federal materials R&D budget. Much of this R&D is not CMP but also includes areas such as metallurgy, ceramics, and materials chemistry. At present, DOE provides 40% of the support for independent researchers in CMP, compared with 35% for NSF and 25% for DOD.

Of the DOE budget for supporting CMP independent researchers, 65% (i.e., $44 million) is spent in DOE laboratories, and only 34% (i.e. $28 million) is spent in universities. In addition, DOE spends another $30 million for construction and operations annually at the laboratories on CMP facilities, which are used by both in-house and academic and industrial researchers. This comprises 75% of the Division of Materials Sciences' MMF construction and operating budget. The large proportion spent on national laboratory independent researcher support has served well to develop good scientific groups and good support for the specialized resources. However, at a time when more jobs are available for CMP graduates than there are graduates, when more students want to work in CMP than there are funds to support them, and especially when academic instrumentation is in serious need of upgrading, we suggest a reconsideration of this partition of resources. In addition, in view of the importance of CMP to the DOD mission, the lopsided distribution of CMP support among the three agencies seems inappropriate, especially in view of DOD's much larger funding base and its the large use of CMP research results and trained manpower.

Independent Research Groups

i. The total increase recommended by the NRC report in independent research funding is desirable and needed; we agree with the size of this increase, and recommend that it be implemented as quickly as possible.

ii. To obtain a distribution between DOE's support of in-house and academic independent researchers that meets the currently observed needs, we recommend that the annual growth rate for the support of academic independent researchers be approximately double that for the in-house independent researchers. This results in almost equal DOE support for the two groups by 1992, and continues a trend established in DOE's funding of solid state physics this past year: the number of university grants was close to 10% greater in FY 1986 than in FY 1985, with an average grant size of $117,000 compared to NSF's $80,000.

iii. Although these recommended increases for CMP funding by DOE are large, by themselves they will not fulfill the needs of the field. To do so, other major sponsors of CMP research, in particular DOD, must increase funding at the rates recommended in the NRC report, especially in view of the fact that DOE bears essentially the whole burden of the major materials facilities.

Major Facilities

iv. The upgrading of existing facilities should have high priority. The addition of experimental halls and instruments and the modification of sources at existing facilities is a cost-effective way of providing opportunities for new frontier science using cold neutrons, synchrotron radiation, and pulsed neutrons. Many central problems in CMP involving structure and dynamics can be answered with cold neutrons; accordingly, guide halls and instrumentation should be provided for cold neutron research ($30M constant dollars over 4 years). These will enable the United States to become competitive in this field in the next decade. Installation of undulators in six of the remaining straight sections of existing storage rings ($16M over 3 years) is important both because these insertion devices will be our premier synchrotron radiation sources for the next 5 years and because they will provide experience for the next generation of synchrotron radiation facilities. An experimental hall and instrumentation ($18M in construction funds over 3 years) in pulsed spallation neutron research is required at the Los Alamos National Laboratory in order for the United States to be competitive, and DOE funding for this facility improvement commenced in FY1987.

v. Major new facilities are required for both synchrotron radiation sources and neutron sources. We concur with the Seitz-Eastman report that the 6-7 GeV synchrotron radiation source should have the highest priority because of its application in several scientific and technology areas. A 6-7 GeV facility incorporating insertion devices will provide orders of magnitude greater brightness than present facilities and will enable the use of smaller samples, as well as providing increased resolution. DOE estimates preconstruction costs of $12M, construction costs of $385M and annual operating costs of $48M (FY 1994 dollars). An advanced neutron source is recommended with fluxes 5-10 times those presently available. This will replace one or more sources constructed in the 1960's and will enable the United States to take a leading position in high-resolution neutron spectroscopy and small-angle scattering. DOE estimates preconstruction costs of about $40M, construction costs around $300M, and incremental annual operating costs of $15-20M. Finally, funds are already in the DOE budget for construction of a 2 GeV synchrotron radiation facility. This will provide VUV capabilities needed for several scientific investigations, as well as for specific DOE defense programs. DOE estimates construction costs of $100M and annual operating costs of $19M.

It is our observation that all of the major materials facilities are heavily used by other fields, including other branches of materials sciences besides CMP as well as disciplines for which heaviest support lies outside the Division of Materials Sciences and the Office of Basic Energy Sciences of DOE (for example, biology and medicine). If the upgrading and new construction needs are deemed to be of sufficient importance to this broader community, an arrangement should be made, at the highest possible management levels, between agencies to fill the requirements on a shorter time scale.

Centers for Specialized Instrumentation

v. Finally, in view of the increasing cost and sophistication of instrumentation for individual researchers, we suggest that the Department in its support of individual researchers consider sponsoring groups of researchers at a university or in some cases centers of intermediate size to which several universities and industries can have access. This is needed because of the large cost of CMP instrumentation, and follows the DOE tradition of establishing accessible centers for such high-cost specialized instrumentation. This mode of sponsoring larger groups of researchers should also be used by DOE to organize its own version of an industry-academic cooperative enterprise. For the CMP field, this effort should be tailored to the size and needs of moderately sized groups in contrast to the Materials Research Laboratories and the Engineering Research Centers of NSF. The present NSF-sponsored Materials Research Groups may provide a model for such a program.

The financial effects of these recommendations are discussed further in Section 4.

2. Importance for the R&D Objectives of DOE

i. CMP contributes to the assurance of energy options both directly through the understanding it provides of the corrosion, embrittlement, high temperature, magnetic, superconducting, catalytic, photovoltaic, and flow properties of materials important to energy technologies, and indirectly through the experimental and process control made possible by the electronics that arise from it.

ii. CMP also plays one of the most important roles in carrying out DOE's responsibilities in health, safety, and environmental impact. Here its contributions to sensors, monitors, controls, and materials knowledge are vital.

iii. As a scientific enterprise, the fundamental discoveries and new concepts that have come from CMP have contributed in an essential way to many other areas of science. Of all of the physics fields, CMP contributes most directly and immediately to technologies with large economic impact. CMP also produces the kind of manpower that is in greatest demand among physics subfields in all parts of the technical world -- academic, industrial, and government. Furthermore, it is one of the most cost-effective of the physics subfields in producing Ph.D.'s. Therefore, CMP should take a high priority for new resources.

The NRC panel designated fifteen topics which are believed to hold high promise for the future. Several of these bear directly on the areas of opportunity noted in a 1984 COSEPUP briefing on physics and on a more recent COSEPUP briefing on thin films and interface science, given in the spring of 1986.(5) What is most clear is that there are overriding needs in all of these areas for new instrumentation and computer support.

This recommendation is perhaps the strongest one that we can make, reinforcing the recommendations of all of the previous panels: university research in CMP must have new instrumentation to produce research and to provide new manpower.

Of the topics highlighted in the NRC report, emphasis, for DOE, may fall more strongly in some than in others. For example a key area is the study of thin films and the interfaces between them and other media. This topic underlies several of the areas of opportunity both scientifically and technologically -- surface studies, artificially structured materials, aspects of polymer and ceramics physics, and strongly interactive systems. Research objectives in these separate topics may well emphasize the thin-film and interface aspects of the topics. Program management in the Division of Material Sciences is well aware of how the areas of opportunity of the NRC report map onto DOE's objectives and needs.

To emphasize the value of Condensed Matter Physics to DOE, we point out that the areas mentioned in the NRC report affect the three main objectives of the DOE research programs: energy, environment and science. For example, studies of materials under extreme conditions, amorphous materials, artifically structured materials, new types of polymers and ceramics (including conducting polymers), new crystalline materials, surfaces and interfaces, non-linear dynamic studies, and theoretical and calculational methods have impacts on almost all areas of the DOE's responsibility to assure energy resources for the nation. For the sensing and control of the environment, devices based on thin films of crystalline and other materials, liquid crystals, and the exploitation of new phase transition phenomena are of vital importance. Measurement techniques such as pico- and femtosecond spectroscopy will also be important in this area. In support of DOE's major responsibilities in basic science, research work on insertion devices (undulators and wigglers) requires support, as does the use of thermal and epithermal neutron sources. For both defense and scientific applications, free-electron laser studies in condensed matter physics are needed. We believe that the recommendations of Section 1b on funding priorities for the support of independent researchers and major materials facilities provide a reasonable framework for pursuing these areas of opportunity.

3. Other Impacts

CMP impacts virtually every field of science and technology since all make use of materials, and since CMP provides the means of understanding and improving the properties of these materials. It has had an especially dramatic impact on every part of the economy which utilizes microelectronics. These include the rapidly growing information sciences and communications industries. The manufacturing sector is increasing its use of microelectronic process control, and as the national economy becomes more service-oriented, the dependence of economic growth on microelectronics will become even stronger. Military defense has spawned much development in this area because of its need for sophisticated sensors, radiation sources, and components for rapid control, surveillance, and guidance.

Although the U.S. microelectronics industry has annual sales of over $100 billion, the nation is in danger of losing its preeminent position. Already, many of the sophisticated physics-based tools for increasing the speed and capacity of chips can only be obtained from foreign sources. Results from research in CMP very rapidly find their way to practical application in chip design and fabrication. It is essential that CMP be adequately funded if the United States is not to risk losing its leadership role in this economically vital technology.

4. Financial Consequences of the ERAB Recommendations

The ERAB Physics Panel recommendations on DOE's CMP budget result in an annual growth rate of about 20% for the first three years. In the 1988-1992 five-year period, we recommend that (1) \$270M (constant dollars) be spent on the support of independent researchers in DOE laboratories, a total increase of 23% over what would be spent in that period at the present level of support; (2) \$220M be spent on the support of independent researchers in academia, a total increase of 55%; and (3) \$380M be spent on the construction and operation of major facilities for CMP. During this 5-year period, the proposed MMF budget would permit the upgrades of existing facilities, the completion of the 2 GeV synchrotron radiation facility, the preconstruction R&D and beginning of construction on the 6-7 GeV advanced photon source, and preconstruction R&D on the advanced neutron source.

5. Impact of a Level Budget for Three Years

If the Department's budget will not permit any growth at all, we suggest that in-house funding be reduced and that the funds made available by this reduction be used to increase the support for academic independent researchers, especially for laboratory instrumentation and new materials synthesis. We would leave it to DOE management to determine whether the reduction in in-house funds should come primarily from a decrease in the in-house independent researcher budget or from a decrease in the major facilities construction and operation budgets. In any event, in a zero-growth scenario any appreciable new construction or upgrading of major facilities would have to be contingent on phasing out existing facilities.

The impact of this no-growth budget would be a serious impairment of an area of science and technology which is critically important to DOE and to the nation. A no-growth budget could well allow other countries to take the lead in a field that contributes greatly to the economy, security, and general well-being of the United States. Of the various subfields of physics, CMP has the largest unmet demand for Ph.D.'s for faculty and industrial positions.(6) In addition, the largest demand for graduate physics study is in CMP.(7) A level budget would further exacerbate this imbalance in physics manpower supply and demand.

C. Elementary Particles, Gravitation, Cosmology and Cosmic-Ray Physics

1. Recommendations Applicable to DOE

a. Recommendations of the NRC Report

i. Elementary Particle Physics

The central goal of elementary particle physics research is to advance the basic understanding of the fundamental constituents of matter and the forces between them. In the past two decades, the progress in high energy physics (HEP) has been brilliant and profound. The nature of the fundamental strong, weak, and electromagnetic forces has been elucidated as deeply for this field as the theory of general relativity has been for gravitational physics. In addition, a very simple "periodic table" of the known fundamental constituents of matter has emerged. All this points toward a clear direction for future research. DOE is the lead agency for federal support of this field of basic research. It provides over 90% of the total funding, operates the three major accelerator laboratories (Brookhaven, Fermilab and SLAC), and provides more than 70% of the support for university groups utilizing these facilities.

The recommendations of the NRC Report pertaining to elementary particle physics can usefully be organized into three categories: general recommendations regarding the near-term future of the field, the recommendation regarding the longer range future, and other specific recommendations.

The general recommendations for the near-term are as follows:

1) The strength and diversity of the community of elementary particle physicists should be preserved.

2) The colliders under construction at SLAC and Fermilab should be completed on schedule, and the budget for experiments and operations at major experimental facilities should assure optimal utilization.

The first part of the latter is now moot, for the Stanford Linear Collider (SLC) and Tevatron are essentially complete. On the question of utilization, the NRC Report notes that existing accelerators are underutilized, and recommends that this pattern not be replicated in the new facilities. They urge that adequate funds be provided for them to assure optimal utilization. Because accelerator laboratories necessarily have large fixed costs, the productivity of the existing accelerator facilities can be increased considerably by a modest increase in equipment and operating funds.

The ongoing program consists of a very large capital investment in very skilled manpower, in existing facilities, and in the new construction initiatives which are nearly complete. The new facilities alone, in particular SLC and Tevatron, represent a capital investment of over $600M (FY87). Of this, about half was the cost of construction, with the remainder roughly equally divided between R&D and equipment (detectors). Adequate exploitation of this program requires a budget approximately 15% above the 1987 level.

The importance of the SLC is that it provides an array of new experimental opportunities, and also constitutes a first attempt at linear collider technology for electron-positron colliders. Linear colliders present a possible pathway to more economical collider technology at much higher energy scales. The Tevatron proton-antiproton collider at Fermilab will, for the next decade, be the highest energy collider in the world. It also has value beyond the high-energy experiments it will accommodate, for it tests numerous design ideas that pave the way for the still higher energy Superconducting Super Collider. Thus, not only do both facilities keep the US at (SLC) or beyond (Tevatron) the capabilities of similar facilities elsewhere in the world, but they also keep the US at the forefront of accelerator design so that the US is positioned to continue to be preeminent in accelerator construction in the future.

The principal recommendation regarding the long-range program is as follows:

3) The Superconducting Super Collider should be expeditiously constructed.

The Superconducting Super Collider (SSC) is a proposed 20 TeV x 20 TeV proton-proton collider, and is the highest priority project for HEP in the future. There is virtual unanimity in the HEP community, here and abroad, that this facility is the best way to attain sufficient energy to address experimentally the most fundamental open problems of the field. Three years of R&D, with over $60M of investment of DOE funds, has produced assurance that the basic technology is at hand. A detailed cost estimate has also emerged which experts in the field, including DOE, regard as firm.

The NRC Subpanel explicitly states that its recommendations are not prioritized. Yet the language surrounding the SSC recommendation suggests that it is in a class by itself(1). It is referred to as the "keystone" and the "centerpiece" of the future of elementary particle physics in the US. Moreover, although the NRC Report endorses the desirability of international cooperation, it does not qualify its endorsement of the SSC with these requirements as does the ERAB Guidelines for DOE Long Term Civilian Research and Development(2). It is fair to say that the NRC Subpanel regards the SSC as part of a minimum necessary national commitment to elementary particle physics; the NRC Panel's endorsement is qualified only to the extent implied by the first two recommendations, that the SSC not undermine the rest of elementary particle physics, or other areas of physics.

The following recommendation is made concerning accelerator design:

4) Strong support should be provided for R&D on accelerator design including support for graduate training in accelerator theory.

Advanced accelerator R&D now represents a small fraction of present funding, despite strong encouragement for growth. To make real progress will require experiments of considerable cost, and the financial impact could become more visible. However, the interplay between these fields and the major HEP research program (experimentation at accelerators and colliders) is complex.

The NRC Report includes the following three general recommendations:

5) Strong support of theoretical research should continue.

The conclusion of the NRC Report is that continued strength in theory will require the development of policies regarding financial support for theorists that permits greater expenditures on computing. At present, DOE support for theoretical research is about $20M/year. In 1984 a DOE sponsored study estimated that the cost of satisfying these emergent computer demands was at most $5-$10M for two Cray-I equivalents, plus one more for SSC computations.

6) Nonaccelerator physics, especially proton decay experiments, should be supported.

The principal recommendation of the NRC Report regarding this field of research is that support for non-accelerator based experiments, especially searches for proton decay be continued. However, in the main text, this recommendation is elaborated to describe the necessity for more elaborate detection strategies. At present, only upgrades of existing detectors are in the DOE plan, and the level of future activity will depend upon the results of the present round of experiments. The principal attraction of continued attack on proton decay is its centrality to an enduring issue in physics: whether all the forces of nature can be unified in a single theory.

7) International cooperation should be maintained and extended.

The NRC Report offers four guidelines regarding international cooperation:

- Some preeminent facilities should always be in the US;
- Nations should cooperate to assure that new facilities are complementary;
- Existing forms of international collaboration should be continued (e.g. facilities should be open to research teams from all nations and teams should be multinational in composition); and
- Nations should engage in more collaboration in planning and building accelerator facilities.

ii. Cosmic Ray Physics

Very little in this field is supported by DOE, as strictly construed, other than a few university contracts. However, DOE-supported proton-decay facilities are very useful cosmic-ray detectors as well. But their support is mainly governed by priorities set within elementary-particle physics research. Some R&D is underway for a large underwater detector (DUMAND). In addition, there exists a proposal for DOE to provide part of the funds for a large underground detector (MACRO) in Italy.

iii. Gravitation and Cosmology

There is increasing linkage between cosmology and elementary particles: historians of the early universe need the concepts and data from elementary particle physics; in return particle physicists gain deeper understanding of their fields. Likewise, new developments in elementary particle theory (e.g. superstring theory) impacts on gravitation research as well.

Funds for major facilities in gravitation and cosmology come principally from NSF and NASA. DOE now provides some university grants and partially supports an astrophysics theory group at Fermilab. While the economic impact of these activities on DOE is minor, attention is needed to foster the cross-disciplinary connections.

Some theoretical and experimental astrophysical work is also pursued at LLL and LANL, especially on topics such as supernova explosions. The dynamics of these processes bears some similarity to that of nuclear explosions, and the computational techniques have some common features.

b. Recommendations by the ERAB Panel

1) Recommendation on the diversity and strength of the HEP Community

The ERAB panel concurs with the NRC Report on the importance of maintaining the strength and diversity of the HEP program. The issue of how much diversity to pursue is complicated. There is a strong case for allocating some fraction of the HEP budget for "diversity," such as non-accelerator experiments, support of older facilities, theoretical research, advanced accelerator R&D, etc. The scientific contribution of the entire field is thereby enhanced by more than the cost of diversity. This argument applies with at least equal strength to the value of diversity in other fields of physics, such as atomic, molecular, optical or condensed matter physics.

2) Recommendation on utilization of existing facilities

ERAB places very high priority on the importance of adequate utilization of the new facilities just completed. The program now in place represents a carefully planned, highly cost effective way for U.S. high energy physics in the next decade to produce unique, forefront science and recapture the leadership which has to considerable extent moved abroad during the first half of this decade. It would be grossly wasteful of public monies not to operate these unique new facilities effectively.

3) Recommendation on the SSC

ERAB recognizes the great scientific importance and broad level of support within the HEP community for this initiative. The excitement for the SSC is easily comprehensible, for it is certain to enable experimental exploration of key unresolved issues in elementary particle physics. Very high energies are required to create and examine particles of very great mass, to detect possible new families of fundamental particles, and to test the alternative theories of the electroweak interactions (perhaps finding the hypothesized Higgs particle). Indeed, the design criteria for the SSC include attaining sufficient energy and luminosity to address these issues decisively.

Reticence of others in the scientific community and in the political sphere to endorse the SSC wholeheartedly is also comprehensible. In an era in which the explicit national policy is to reduce dramatically the role of the federal government in many areas, the budgetary implications of the SSC are very large, regardless of its scientific merit. At a price of approximately $3 billion in 1987 dollars (not including research equipment, preconstruction R&D, and site acquisition), the SSC would entail an approximate doubling of total federal support for elementary particle physics during the years of peak expenditures on its construction.

Thus the SSC is in a class by itself in terms of expense. To provide the SSC from the current particle physics budget would eliminate not only other worthy, less costly proposals, but nearly all of the existing research program in elementary particle physics for approximately a decade. No one -- not even the most ardent SSC proponent -- would propose so draconian a policy. The plain fact is that the SSC cannot be undertaken unless there is a multibillion dollar incremental commitment over the next decade.

Past ERAB recommendations that the SSC be done in cooperation with others remain valid, and should be pursued. But it would be unrealistic to make support for the SSC contingent on obtaining such commitments. If the SSC is to be located in the United States, or even Canada, most of the funds are likely to have to come from the US. The advantages of proximity and control are significant; a nation must expect to have to pay for them. Indeed, a most cogent argument for expeditious commitment to the SSC is to keep the US preeminent in accelerator technology and experimentation, a rationale that will not be lost on European and Japanese scientists or statesmen. Moreover, even if collaboration is arranged for the SSC, the quid pro quo is likely to be increased US financial support for facilities elsewhere, so that the net budgetary impact of cooperation will not necessarily be salutory.

We recommend that funding for SSC construction be considered in a national context which is larger than that of DOE alone--this recommendation is in line with previous ERAB policy statements on the SSC such as that contained in "Guidelines for DOE Long-Term Civilian Research and Development Vol. I"(2), which recommended funding subject to the requirement that other R&D priorities within DOE not be preempted. The funding of the SSC should not undermine other important areas of physics or, indeed, other areas of science.

The issue has been raised as to whether the scientific priority should be for a construction start on the SSC now or for R&D leading toward a TeV electron-positron linear collider. The difficulty of this latter approach is that new technologies being explored cannot match the scientific capability and technological readiness of the SSC for a least a decade. Furthermore, there is no persuasive evidence that those new technologies now under study will ever lead to costs lower than the present SSC proposal. Thus, the issue of authorizing the SSC is whether the quest towards higher and higher energy to study nature in smaller and smaller dimensions will continue in the U.S. beyond the Tevatron's lifetime.

4) Recommendations on advanced accelerator R&D, theoretical research, nonaccelerator physics, cosmic ray physics, gravitation and cosmology.

The ERAB Panel recommends that the funding and relative priorities be determined via the existing DOE peer-review mechanisms.

5) Recommendation on international cooperation.

Successful implementation of the NRC recommendation would somewhat ameliorate the budgetary implications of the first three recommendations. Because expensive facilities of one type or another are virtually always under construction somewhere, international sharing of the burden of planning, construction and operation would significantly smooth the budgetary requirements of maintaining excellence in the field, not only in the US but elsewhere. Good coordination reduces unnecessary duplication of facilities and experiments, and while the NRC Report provides no recommendations as to how this objective can be attained, a quite extensive network of international consultative mechanisms and traditions is already in place. And although there exists no international authority regarding new facilities, economic realities alone are already becoming very effective in avoiding duplication.

International collaboration has been an important element in high-energy physics planning and practice, and will continue to grow in importance. It is an essential element with respect to the SSC project. DOE has been sensitive to these issues; the NRC Panel recommendations are in line with existing policy.

2. Importance for the R&D Objectives of DOE

A principal mission of DOE is to "advance fundamental knowledge and education in the disciplines it supports." In these terms the DOE HEP program has been extremely successful. In looking to the near future, this successful pattern will be continued only by exploitation of the capital investments made in the last 5-10 years in machines which are now coming on line. In terms of the far future (1995 onward), the HEP community asserts that it is necessary to go forward with the SSC.

HEP provides a pool of highly skilled and productive manpower, with talents characteristic not only of the research scientist, but also with managerial skills and the talents associated with working successfully on big projects containing large groups of engineers and technicians. About 50% of HEP-trained experimentalists remain in the field. The remainder, much sought-after by industry, readily adapt to a variety of other fields.

3. Other Impacts

More than other areas of physics, research in elementary particle physics and the related areas of cosmic rays, cosmology, and astrophysics are distant from obvious direct short-term payoffs to industry, national defense, or other national goals beyond the pure pursuit of scientific knowledge. However, research of this nature has always led to major long-term benefits which only became evident after the passage of decades. In addition, there are a number of identifiable spillover benefits.

First are the benefits to other areas of physics. Elementary particle physics is closely related to astrophysics and cosmology, for it deals with energy scales and forms of matter that existed naturally only in the first moments after the birth of the universe.

Also, particle accelerators developed for basic research have found very significant practical applications. Electron storage rings originally built for basic HEP research have evolved into synchrotron light sources now in demand by industry and a host of research fields, from chemistry and condensed matter physics to biology and medicine. From this development alone, one may argue that HEP has been highly effective in terms of contributions to applied research. Use of particle accelerators in medicine continues to grow in importance. Electron, proton, neutron, and ion beams are routinely used in clinical practice. And R&D on high-gradient linear acceleration or other advanced acceleration concepts may potentially have military applications.

The large scale use of superconductivity in particle accelerators has stimulated industrial advances in superconducting technology, for example in finding better and cheaper ways to manufacture superconducting wire of unprecedented quality. In the long run, development of superconducting technology may lead to important impacts in the fields of electrical power generation, transmission, and use, in large-scale energy storage, in use of superconducting magnets for separating materials, and in high speed ground transportation.

Experimentation in HEP demands acquisition and processing of large amounts of data at high speed. The technology is novel and state-of-the-art. A considerable R&D interaction with the electronics industry is leading to the development of unique devices and processor architectures which have potentially broad applicability to other fields. Large-scale computations in theoretical HEP likewise are supporting development of novel computer architectures, some at the hardware level. These again promise to have broad applicability to many fields of pure and applied science.

4. Financial Consequences of the ERAB Recommendations

1) Existing facilities

Total DOE expenditures for HEP, after falling from a level of $800M (in FY87 $) in 1970 to $450M in 1975, have remained between $500M and $600M from 1977 to 1985. The 1986 budget was $490M and the 1987 Continuing Resolution budget is $525M. In order to implement the proposed program (not including SSC construction), this budget would have to be augmented by 18% above the FY87 budget. At present, there is relatively little new construction money in the DOE HEP budget. The AGS booster at BNL and a new computer facility at Fermilab are the major items underway, with about $45M yet to be spent. Major capital equipment items in coming years are the large detectors for experiments on the SLC and Tevatron (SLD at SLAC, D0 at Fermilab). These projects are underway, but there is $85M yet to be spent. In addition, a large number of fixed-target experiments at FNAL and BNL, as well as construction of underground detectors, represents additional equipment costs estimated to be about $15M/year.

About 40% of the HEP budget goes for facility operations. This is insufficient for optimum use of existing facilities. Because of the recent completion of major construction projects, there is immediate need at SLAC and

FNAL for increases in operating funds in order to run their new facilities. Optimum utilization of all facilities would require an increase in expenditures of about $35M per year.

2) SSC

The financial impact of the SSC is, of course, enormous. This is an initiative of unprecedented size and cost and requires major additional funds beyond the recent pattern of DOE budgets. The estimated construction cost is 3 billion (FY87) dollars and the estimated cost of research equipment, preconstruction R&D, and site acquisition is 1 billion dollars. This implies an approximate doubling of the US expenditure in HEP over about a decade. After completion of the construction phase of the SSC, the HEP budget is expected to return to approximately the present recommended level, with the annual budget for the SSC component estimated to be $250M (FY1987 dollars). This is only possible if facilities in the present program are closed or used for other purposes. Candidates include PEP, SPEAR, Tevatron Collider, and AGS. All four of these could still be used for other purposes. PEP and SPEAR are excellent sources of synchrotron radiation, the Tevatron is a potential injector for the SSC, were it to be sited at Fermilab, and the AGS will serve as the injector to RHIC.

3) Theoretical research, non-accelerator experiments, advanced accelerator R&D, and international collaboration

The financial impact on DOE of theoretical research and non-accelerator projects has been moderate and is likely to continue to be so. The annual cost is roughly $20M for theoretical research and $10M for non-accelerator experimentation. Advanced accelerator R&D should show real growth in the next decade. At present the budget is about $10M/year, and an increase to approximately $20M/year is projected. Increased international collaboration to enhance the prospects of international contributions to the SSC is very attractive economically. To achieve this objective, however, will require considerable efforts to convince others that the US is a reliable long-term partner in all aspects of HEP, including projects underway in the US or elsewhere. This implies a positive but uncertain increment to the HEP budget.

5. Impact of a Level Budget for Three Years

1) The ongoing program

At present levels of support, university HEP groups are in considerable difficulty. University support is a small fraction (16%) of the total HEP budget, and this deficiency is already creating a deep and long-lasting impact on the health of the field.

Two forefront facilities (the SLC at SLAC and Tevatron at Fermilab), are coming on-line. They promise to restore a great deal of the US leadership that was lost in the first half of this decade. Insufficient support of these programs will mean a loss to science as well as a major lost opportunity in maintaining the traditional leadership role played by US scientists. In addition, these programs have attracted significent participation by European and Japanese physicists. Lack of adequate support would not only greatly delay and compromise the proper execution of these experiments, but also would constitute a repudiation of our international commitments. The implication of these observations is that a level budget for several years probably will require the premature closure of other, older facilities in order to retain the viability of university programs and to make funds available for the new facilities.

2) Construction of the SSC

It may be expected that with adequate support the US program which is now in place will be strong throughout the remainder of the decade. However, this preeminence will be expected to fade in the early 1990's as new European facilities (HERA, LEP, UNK) are commissioned. The keystone of the long-range plan for US high energy physics through the 1990's is the SSC. According to the NRC report, it is central to the future of elementary particle research in the US and essential for confronting the fundamental scientific issues facing the field. A level budget implies that the SSC will not be built, or will be long delayed. This would cripple the US program and lead, perhaps indefinitely, to the major forefront facilities being abroad.

The impact of delays in SSC construction deserves special attention. Stretchouts would increase the project cost. The NRC Report states:

> "The optimum duration of the (SSC) construction period should itself be an object of study... It will depend on many factors... Finding ways for minimizing the delay between start of construction and first use for physics research must be given great emphasis."

3) The remaining activities

The health of theoretical research, non-accelerator experiments, and advanced accelerator R&D depends on the overall health within elementary-particle physics. For example it is difficult to maintain a vigorous program in advanced accelerator R&D if there is inadequate support for the existing accelerators.

As noted above, inadequate support of the existing program implies a failure of the US to honor international commitments. In addition, international collaboration is generally viewed as an essential element of the SSC initiative. A lack of resolve in this area implies lost opportunities in science, in the health of the US program, and in international relations.

D. Nuclear Physics

1. Recommendations Applicable to DOE

a. Recommendations of the NRC Report

Nuclear physics is the science concerned with the properties of nuclear matter. It faces substantial intellectual challenges and important questions that are being addressed as aggressively as possible under the present very tight budgets. The NRC report of this field identifies areas with great promise for future growth and discovery. It is a field in which the Department of Energy has a 'unique mandate' for the support of basic research, including the construction and operation of the major facilities needed. The Nuclear Science Advisory Committee to the Department of Energy and the National Science Foundation (NSAC) prepared a Long Range Plan(8) in close coordination with the NRC Committee. The NRC report and the NSAC Long Range Plan recommend a scientific program and priorities on facilities for nuclear physics that are in substantial agreement.

Two new major facilities were identified in the NRC report. They both are designed to explore the implications of the quark-gluon description of the "elementary" proton, neutron, and meson constituents of the nuclear many-body medium. One of these facilities is an electron accelerator with continuous electron beams, needed to provide more detailed and more accurate data on the nuclear interior. Construction funds for starting such a facility (CEBAF) have been included in the FY87 budget request. The other major facility is a relativistic heavy-ion collider. Such an accelerator is needed to explore the possibility that, under extremes of energy density, nuclear matter may undergo a phase transition into what may be an entirely new phase of matter. The conditions reached in the collision of two energetic nuclei will produce very high energy densities over a relatively large volume of space, sufficient to lead to the formation of a possible 'quark-gluon plasma', a unique form of matter that has not existed since the first instants of the universe.

Other recommendations concern the overall health of the field and point to opportunities for upgrading existing facilities or adding new ones in the more distant future. It was estimated that a reasonable fraction of these upgrading opportunities could be handled under an ongoing program of accelerator improvements and would not cause perturbations in the overall budget. A major point is made of the need to upgrade the instrumentation in the field; it has fallen severely and very detrimentally behind the state of the art because of continuing budget stringencies and uncertainties. The importance of strengthening support for theory in the field, and of providing access to super-computers, is emphasized. The need to attract and train first-rate people in this field, as in other fields of basic physics, is stressed as a vital recommendation. And the final recommendations comment on the need for a supply of stable isotopes and for data compilations.

b. Recommendations by the ERAB Panel

The Panel endorses the NRC recommendations described above. The relative priority ordering of the two major construction items, with CEBAF followed by the Relativistic Heavy-Ion Collider, was set by NSAC and by the NRC committee; we concur with that order, and CEBAF is indeed already in the FY87 budget. Of the remaining recommendations, the Panel wishes to stress the vital importance of renewing instrumentation with the relevant R&D and of training young scientists, essential for both the basic science and its contribution to the more general objectives of DOE and the nation. While the budgetary impact of these latter initiatives is relatively minor, their impact on the vitality of the field can be considerable.

2. Importance for the R&D Objectives of DOE

The understanding of the properties of nuclei and nuclear matter is both an important objective of basic scientific inquiry and a source of current and potential applications. The nucleus and nuclear processes are responsible for our sources of energy, either indirectly through the Sun, whose energy output supports all life on Earth and is responsible for our fossil fuel resources, or, more directly, in terms of man-made fission or fusion. Nuclear physics and the techniques of nuclear research have also spawned many applications outside of the uses of fission, from nuclear medicine to isotope tracer methods, and the use of accelerators for a variety of other applications. Nuclear physics is clearly relevant to a number of the applied R&D objectives of DOE.

To quote the NRC report: "Nuclear physics presents a remarkable paradox: its awesome technological progeny, nuclear power and nuclear weapons, are among the most well-known and hotly debated topics of our age, yet the physics of the nucleus itself is possibly the least understood of the basic sciences. The fundamental understanding of atomic nuclei has progressed considerably in the decades since the Manhattan project. Important issues in nuclear structure and dynamics are being addressed with some of the existing facilities. A major frontier area in urgent need of exploration and understanding is to establish the connection between the older description of nuclei as collections of protons and neutrons and the implications of the new understanding of these particles in terms of quarks and gluons that has emerged from the discoveries of elementary-particle physics. The exploration of this interface holds the potential of shedding new light on the understanding of nuclei and nuclear forces and also of allowing a new window on the regime of quarks and gluons in the many-body context of nuclear matter. This area of fundamental knowledge in the United States depends very largely (75-80%) on DOE support."

The training of scientists in nuclear physics is a clear and pressing need that goes well beyond the needs of basic research in the field. Scientists trained in nuclear physics are contributing widely to a number of R&D areas, including nuclear medicine, energy production, national defense, and many fields of technology. This trend is continuing among new generations of scientists and there is every reason to believe that the contributions of the science and of this manpower to the future scientific and technological needs of the nation will continue to be essential.

3. Other Impacts

The impacts on defense and technology have already been mentioned, and are discussed further in the NRC report and the NSAC Long Range Plan.

DOE support for nuclear physics in 1986 has risen by 40% in current dollars since 1980. In constant dollars the support has remained about level, consistent with the overall DOE support of Energy Sciences budgets. As a consequence, nuclear physics in the U.S. is losing ground relative to the rest of the world.

The U.S. international scientific leadership of the 1960's and 70's has deteriorated, since the rate of investment in basic research in this field in the U.S. (in terms of fraction of GNP) has fallen a factor of about three below the rate in other technologically advanced countries. The rate of construction of new facilities, the development and general state of instrumentation, and the number of new researchers entering the field have all fallen behind in the U.S. The consequent loss of leadership in the field is now evident in a number of subfields. While the U.S. still perhaps retains overall leadership compared to any one country, even the overall efforts in some, much smaller countries (e.g. Germany and France) are getting to be competitive in some aspects with the U.S. effort. This is the inevitable consequence of the steady decline in support for basic research in the U.S. over the past 20 years, and a concerted effort needs to be exerted to keep it from deteriorating further.

The relevance of nuclear physics to other sciences is manifold. Since the major sources of energy originate in nuclear processes in the sun and the stars, and the properties of stellar matter are governed primarily by nuclear interactions identical, or closely related to, those in atomic nuclei, nuclear physics has a very close and productive link with astrophysics. The applications of nuclear techniques, accelerators, detectors, radioactive isotopes etc. have had profound effects in biological research, atomic physics, geology, and materials science. The many-body nature of nuclear matter provides close links to the theory of condensed matter, and the nature of the basic interaction in nuclei and the opportunities for testing basic symmetries in the nucleus with the lower-energy accelerators of nuclear physics, make for an especially close intellectual link with particle physics. Basic work in nuclear physics has had and is having major impact on other scientific disciplines and there is every indication that such impact will continue in the future.

4. Financial Consequences of the ERAB Recommendations

The construction and capital equipment costs of the NRC recommendations are primarily for two major facilities. The construction of CEBAF is under way and it is estimated at $236 million (plus $26 million supporting R&D) over a period of five years. The total costs include the first generation of major experimental equipment items for these facilities. The recommendations on upgrading present facilities will have some impact on the corresponding DOE budget categories, on the order of an incremental $10 million per annum.

The projection of annual operating costs for the new facilities is $27 million for CEBAF and $50 million for the relativistic heavy-ion collider.

The operating costs associated with the recommendations for support for improvements in instrumentation would require $10 million incremental, annual operating funds on a continuing basis, and considerable headway could be made on the recommendations on manpower with $8 million per annum, in both theory and experimental research.

The life span of major facilities is difficult to estimate, but from experience it would seem to be on the order of 10-15 years, or possibly as great as 20-25 years with major innovative upgrades.

The NSAC Long Range Plan estimated that the overall budget for the field would increase by about 30% in real terms by the time the heavy-ion collider was complete, in order to operate the new facilities and to keep the best of the existing facilities productive.

The NSAC report lists nine current U.S. "user facilities", seven of which are supported by DOE. There are also roughly a dozen "dedicated university facilities," three of which are supported by DOE. Over 90% of the DOE budget is directed towards the user facilities, the largest two being the U.S. meson factory LAMPF and the first relativistic heavy-ion accelerator in the world, the BEVALAC; these two require over half the operating budgets in DOE nuclear physics. The support for a large number of nuclear physics facilities (about 30) has been terminated over the past decade. One of the above-mentioned DOE user facilities was also shut down since the NSAC study a little over two years ago. The examination of the scientific priorities of the field is a continuing process and there is little question that, as the new facilities come into operation, some of the major existing ones will have to be phased out.

5. Impact of a Level Budget for Three Years

There is no question that, even in terms of constant dollars, the cost of doing the same quantity of advanced research depends upon the ability to obtain higher particle energies, which requires more expensive accelerators. A level budget, therefore, has negative consequences for nuclear physics since a major portion of the expenditures lies in the construction and operation of accelerators. For these reasons, a constant 3-year budget in nuclear physics represents a major decrease in research capabilities. Clearly the completion of CEBAF would have to be delayed. In addition, a reexamination of all current facilities would have to be made to determine which major facilities should be phased out in order to allow even the stretched-out CEBAF schedule to proceed.

While such adjustments obviously can be made, they would inevitably result in a large loss in nuclear physics capabilities, relative to the international effort. Additional decreases in other areas of nuclear physics, required to sustain the construction of CEBAF on a stretched-out schedule, would further harm the U.S. position. The future consequences to nuclear physics, for which DOE has special responsibility, would have to be weighed against losses in fields where funding is obtainable from other sources in the U.S. government. Nuclear physics has clearly had an enormous recent impact on our defense system as well as our national welfare. While the precise nature and the importance of future impacts may not be predicted with any accuracy, there is no question that a level budget would substantially reduce the potential future benefits to be derived from research and training of scientists in this field.

E. Plasma and Fluid Physics

1. Recommendations Applicable to DOE

a. Recommendations of the NRC Report

i. Plasma Physics

The primary reason the DOE supports plasma physics research is that plasma physics is basic to achieving a controlled fusion reactor. The Panel on Plasma Physics of the NRC Physics Survey recognized this connection, and based many of its recommendations on the substantial significant advances of fusion related plasma research over the past decade. These advances include increased precision of experimental and theoretical techniques, revolutionary numerical computations, precise diagnostics and measurement, and development of the theory of nonlinear plasma physics as well as the achievement of reactor-like values of plasma density, temperature, and confinement time. Based on these advances and achievements, the NRC Panel recommended that the federal government proceed with the next generation of major projects in fusion.

In addition to direct application to progress toward a fusion reactor, the NRC Panel cites the role of plasma physics programs in the development of a scientific discipline basic to defense programs and several areas of advanced technology, as well as in the training of plasma scientists for other DOE research and development needs involving modern classical physics and modeling. Based on these needs, the NRC Panel recommended that the federal government maintain a renewed commitment to basic plasma physics research.

The NRC Panel recommended marked increases over 1984 levels in the DOE fusion research budget, both for magnetic and inertial confinement fusion research, and that the emphasis in program planning center on the following areas of activity:

1) Because fundamental understanding of plasma properties precedes the discovery of new applications, and because basic plasma research can be expected to lead to exciting new discoveries, increased support for basic research in plasma physics is strongly recommended. The physics community should be encouraged to submit high-quality proposals for basic research in laboratory plasmas.

2) Because the science of plasma confinement has reached a stage of development which enables the design of a new generation of facilities which can achieve reactor-like values of density, temperature, and confinement time, it is recommended that to advance our understanding of fusion, and to maintain and extend U.S. excellence in plasma physics, the federal government proceed with the next generation of major projects in fusion research. Specifically, the following is recommended:

- o Initiation of a moderate-cost tokamak experimental facility (less than $1 billion plant and capital expenditures) designed to achieve plasma ignition and study confinement and control of a burning plasma.
- o Depending on future assessments of the tandem-mirror data base, potential

utilization of upgraded mirror facilities to test fusion blanket and engineering components.

- o Vigorous pursuit of a broad-base program in magnetic-confinement research, encompassing tokamaks, mirrors, stellarators, bumpy tori, reversed-field pinches, and compact toroids.

3) Use of present driver facilities to determine the physics and scaling of energy transport and fluid and plasma instabilities to regimes characteristic of high-gain targets.

4) Use of the new generation of drivers under construction to implode D-T fuel mixtures to 1000 times liquid density required for high-gain targets and to implode scale models of high-gain targets to the density and temperature of the full-scale target.

5) Identification and development of cost-effective, multimegajoule driver approaches.

ii. Fluid Physics

The report on Fluid Physics outlines in detail the research being done in fluid physics, describes briefly sources of federal support, and makes general recommendations that are largely of a qualitative nature. The general recommendations that are of interest to DOE are the following:

1) Targeted research should be initiated to develop instrumentation for essentially simultaneous, multipoint measurements of flow properties throughout large volumes.

2) Funds and organizational mechanisms should be provided to make unique national fluid-physics facilities available to university and nongovernment communities for basic research.

3) Support should be provided for the expansion of analytical methods for solving complex flow problems (e.g. turbulent flow). Primarily, time on large computer systems is required.

The report provides a detailed discussion of the areas of fluid research considered to be of current importance. From that listing, the areas that are also of interest to DOE are the following:

1) Of special interest to DOE's mission:

-combustion and reacting flows
-stability
-turbulence
-radiation hydrodynamics
-flow through porous media
-multiphase flow

2) Of general interest:

-non-Newtonian flows and rheology
-molecular and statistical phenomena
-rotating flow phenomena
-sound generation and propagation

b. Recommendations by the ERAB Panel

i. Plasma Physics

Both because of direct relevance to the DOE's energy supply mission and of the development of a field of physics basic to other DOE and government programs, the ERAB Physics Panel agrees with the general recommendations of the NRC Panel. In specific detail, however, the situation has changed since the 1984 summary.

Despite clear advances, achievements, and contributions to other federal programs, DOE funding of plasma and fusion physics has decreased sharply since the NRC Panel report in 1984, with a reduction of 23% from 1984 to 1986 in funding for the magnetic confinement programs, and a reduction of 33% from 1981 to 1986 in the inertial confinement program. This has already resulted in a curtailment or elimination of DOE programs cited and recommended by the NRC survey. This has included, in addition to fusion projects, a perceptible erosion in basic plasma physics, which formerly developed in a natural way in response to program needs and the growth of the mainstream program. In a steady or declining program, the continued use and exploitation of existing facilities virtually forces a reduction in development of the physics base in favor of the more direct and visible applied achievements of the program.

Therefore, in light of the existing funding level and program status forced by the 1984-1986 reductions, we both add to and alter the NRC survey recommendations, as follows:

1) The DOE should return the budget for fusion research to a level somewhat above the FY1984 levels, by establishing a stabilized funding projection which restores the budget for magnetic fusion to the $570 million per year level, with the increase from FY 1987 taking place in a staged fashion for FY 1988-90.

This level will allow fusion research activity to proceed in a reasonable way, progressing toward the applied goals of the program. This progress requires steps toward both achieving reactor conditions and establishing approaches to fusion which might have ultimate economic advantages. These steps can be taken within the budget recommended above, because of the achievement of the fusion community since 1983 in defining a facility, the Compact Ignition Tokamak (CIT), which is substantially more cost-effective, less costly, and carries less technical risk than engineering test reactor facilities which combine ignition parameters with high-duty-cycle, long-pulse operation. This staged approach toward a fusion reactor is also consistent with present perceptions of energy needs. In particular, we recommend that the stabilized budget be used to:

2) Initiate a tokamak experimental facility of the CIT type (costing approximately $400 million), designed to perform a D-T ignition physics test, study confinement and control of a burning tokamak plasma, and provide a data base for design of an engineering test reactor.

3) Vigorously pursue a broad program in basic magnetic confinement research, encompassing advanced and alternative concepts as well as tokamaks.

4) Review the implications of the program plan on basic plasma physics research, to determine whether separate funding and management must be set in place to ensure that these basic needs are satisfied.

In the inertial confinement program, in addition to exploitation and analysis of existing facilities and results, equal emphasis should be given to the following:

1) Use the new generation of drivers under construction to implode D-T fuel mixtures to 1000 times liquid density required for high-gain targets and to implode scale models of high-gain targets to the density and temperature of the full-scale target.

2) Identify and develop cost-effective, multimegajoule driver approaches.

A vigorous mainline research program is essential to technical progress in both magnetic and inertical confinement research. Moderate-size experimental facilities are the primary sources of the scientific and technological innovations required to develop fusion to its fullest potential.

Continued research on alternate fusion concepts is essential to advance basic understanding of plasma confinement and to foster the development of approaches that show significant promise of improved reactor configurations.

Intensive research must continue on the theoretical and computational descriptions of magnetically and inertially confined plasmas and on supporting experiments in basic plasma physics. These have been a source of many promising new concepts in fusion research.

Continued strong university involvement will be essential to fusion research for the foreseeable future. Universities augment fusion research in the national laboratories in several unique and important ways. They educate and train professional fusion researchers; they provide the fusion program access to a breadth of talent and intellect in the sciences and engineering; and their research is a major source of innovative ideas and scientific and technological advances.

If the present trend (or indeed even the present level) of funding continues, the NRC survey recommendations will be moot. Plasma physics and fusion research will doubtless survive, but will not exhibit the orderly progression implied by the NRC survey analysis of this important and practical discipline.

ii. Fluid Physics

The ERAB Panel agrees with the general recommendations set forth in the NRC report. It is sensitive to the figures in the report that indicate the support of fluid physics by DOE to be less than 2% of the total federal funding for fluid physics. Currently, the support for fluids research is estimated to be $4.4 million with only $1.5 million indicated for general research supported by Engineering Science.

The panel makes the following specific recommendation for DOE support for fluid flow phenomena:

1) As fluid physics is a basic science of importance to a variety of areas supported by DOE, and as the current support of fluid physics by DOE is marginal, an increase in support of certain key areas seems justified. To that end, the panel recommends an increase in funds of $6 million over the period from 1988 to 1992 to be equally distributed over the six major areas of special interest set forth in section 1a.

The panel also makes two other recommendations that do not have significant financial consequences:

2) DOE should explore ways in which use of facilities for fluids research might be increased.

3) Availability of time on large computer systems for fluids research should be examined.

2. Importance for the R&D Objectives of DOE

i) Plasma Physics

1) Energy Supply

In a more direct way than possibly any other Office of Energy Research program, plasma physics research supported by the DOE is a dedicated applied program, whose aim is to provide a unique energy option for the United States and the world.

Thermonuclear fusion is one of the very few options available that can provide for mankind's energy needs in the very long term. Based on essentially inexhaustible (billion-year) fuel reserves of near-zero cost, fusion power is perceived to offer many advantages over alternatives, such as solar power or the breeder reactor. Environmentally, fusion has the potential to provide a much safer system than the breeder reactor, with respect both to the safety of the plant itself and to all aspects of its fuel cycle: fissionable materials are not involved; fusion's "ashes" are inert; and radioactivity associated with plant operation can be minimized and made to be short-lived.

Although the study of naturally occurring high-temperature plasmas is of considerable scientific interest of itself, it has been the quest for controlled fusion power that has been the dominant influence on research in plasma confinement and heating for three decades. This quest has proven to require a deep understanding of the physics of the plasma state, and has driven a period of rapid development of this field, as noted by the NRC survey. There seems now to be a substantial and reliable experimental basis for the detailed description of the fundamental scientific requirements of a fusion reactor. Thus the NRC recommendations are based both on the energy implications of plasma research, and on the progress of that research in the applied area of thermonuclear fusion.

2) Mitigating the Impact of Energy Production on Health, Safety, and the Environment

All energy production schemes include risks and ecological impacts; fusion has the potential to offer substantial advantages compared with other forms of nuclear energy. The sharp reduction in nuclear waste, the reduction in long lived radioactive fuel inventory, the fragile and easily quenched nature of the fusion process, and the potential inherent safety against loss of coolant damage are examples which have already been studied and cited as environmentally attractive features of a fusion reactor.

3) Manpower Needs and Basic Knowledge in DOE-Supported Disciplines

Although its direct contribution to the energy supply issue is of prime importance in relating the NRC survey recommendations to DOE planning, the importance of plasma physics in supplying manpower needs and advancing the knowledge base of U.S. science and technology programs is also recognized. In terms of manpower, 80% of plasma physicists are engaged in defense-related R&D; virtually none are produced by these programs. In terms of disciplines based on basic plasma physics accomplishments, there are a large number. For one, the study of intense beams of ions, electrons, and photons involves collective effects which place the subject in plasma physics rather than accelerator or particle physics. This study was initially directed toward simulation of nuclear effects, but has since led toward light-ion inertial confinement fusion, heating and current drive in magnetic confinement fusion, microwave generation, a number of distinct collective accelerators, laser pumping, x-ray sources, directed energy weapons, and free-electron laser radiation sources for spectroscopy, accelerators, radar and fusion application. Related advances in pulsed power technology and ion diodes which also follow from basic plasma physics research are of importance to DOE programs.

The pace and direction of plasma physics development has been dictated by the needs of the fusion reactor program. It is fortunate that those needs were broad and basic. This has led to a well balanced development of virtually all aspects of plasma physics. As the fusion program becomes increasingly successful in its energy related goals, and particularly as budget constraints become more severe, the nearly automatic fulfillment of the manpower and scientific development needs for plasma physics can be expected to diminish, and a conscious effort will have to be made to determine the appropriate level and scope of basic plasma science training and development.

ii. Fluid Physics

There are few problems in energy research that do not involve an understanding of flow phenomena. The flow of fluids through pipe networks, the combustion of fuels, the flow of liquid hydrocarbons through porous media, the use of biological products for energy, the mechanism of heat transfer, and the behavior of plasmas in energy applications are all examples of areas in which an understanding of flow phenomena is of critical importance. Fluid physics is a basic scientific discipline. Support of both basic and applied research in flow phenomena is great importance to DOE research.

3. Other Impacts

i. Plasma Physics

In addition to energy production, the NRC recommendations were related to the national economic and technical base, the defense position, and the role of the U.S. in international scientific leadership. This latter is by no means simply a matter of national pride. Leadership in a physics area which produces energy and neutrons gives so much economic and defense leverage that the program was classified for the first ten years of its existence. The survey states, "if the United States' pre-eminent position in the world-wide fusion program is to be maintained into the 1990s in the face of aggressive Japanese and European competition, the pace of new-device authorization that characterized the early 1970s must be restored soon." A similar statement applies to the inertial confinement fusion program, where "The United States has maintained world leadership in inertial-confinement fusion research since its inception in the late 1960s. Its near-term applications are military, with promising long-term applications to energy production." Timely execution of the recommendations is predicted to "provide the basis for a decision in the late 1980s on the next generation of experimental facilities. Drivers in excess of a megajoule would allow demonstration of high-gain targets for both military and energy applications."

ii. Fluid Physics

Because fluid mechanics undergirds so many other research areas, the possibility of widespread applications of research is evident. For example, a better understanding of turbulence would have immediate application to such diverse fields as meteorology, aerodynamics, and combustion phenomena. It is an area that cannot be overlooked in either science or engineering.

4. Financial Consequences of the ERAB Recommendations

i. Plasma Physics

The cost of a fusion program compatible with its potential value for the energy, defense, and manpower needs of the nation is not small. The recommendations of an initial change in the budget to at least the 1984 level would require an increase of $110 million for magnetic confinement and $70 million for inertial confinement, compared with 1986 budgets. This is primarily due to the substantial distance fusion has progressed toward its goal: as reactor conditions are approached, experiments become increasingly reactor-like in size, cost, and complexity. The dominant capital item in the recommended program plan is the ignition/long-burn-time experiment, which might require of order $0.5-1 billion over a ten-year time period. Further capital expenditures would depend on progress on alternative concept devices now operating or under construction.

ii. Fluid Physics

The financial consequences of increasing the budget for fluids research by $6 million over a five year period are relatively minor considering the importance of the field, the possible pay-off to other fields, and the limited funding that is currently supporting fluids research. At a minimum, the support currently in place should not be reduced.

5. Impact of a Level Budget for Three Years

i. Plasma Physics

In 1984 the fusion program, riding the crest of impressive successes in plasma confinement, conditioning, and understanding, envisioned constructing a new round of devices designed to extend the science into regimes of near reactor conditions, where unique problems of physics and technology might be faced and attacked with the techniques and understanding developed over the past two decades. These new regimes included exploration of confinement geometries less fully understood than the mainline approach, but which promised significant practical advantages for reactor performance. The NRC survey confirmed and recommended this plan and the moderate budget increase it implied. Instead, the budget has been cut from the 1984 level, by about 25% in actual dollars. The FY1984 budget is the "level budget" assumed for discussion in this section.

The present budget level for plasma physics and controlled fusion research is not adequate to achieve the NRC recommended goals, at any pace. Even the consequences of a several year 1986-level budget will be pronounced and perhaps irreversible. The program required to achieve the DOE goals can be summarized as follows:

1) Advance the mainline fusion concepts to reactor-like conditions.

2) Pursue a broad program in alternate approaches to a fusion reactor.

3) Maintain a strong program of basic research in plasma physics.

At the present budget level, these three elements strongly compete for funds. The need to advance the mainline is clearly preeminent in a mission oriented program, and program plans since the NRC survey have centered on how to do this in the face of sharp budget reductions. The result, already, has been to force stark choices in the program base and in basic research. This has led to a noticeable erosion of the basic research program, and a narrowing of the base of the alternate approach program. From 1986 to 1987, university research was reduced by 17%. Over the past several years, even relatively inexpensive projects designed to answer key physics questions about non-mainline concepts have been terminated. Less visible but equally important is the fact that interest in proposing new ideas is vanishing in the face of sure rejection. If level budgets persist (at the 1986 level) this trend will also persist. The basic plasma research program will without doubt be most severely affected, based on historical trends and on pressures for near-term results. A five-year decline in such programs will sharply reduce the manpower which now staffs military and space applications of plasma physics, as well as reduce the basic accomplishments which lead to future advances in technology. The mainline and alternate concept programs will also shrink somewhat in scope and number; over a two or three year period this might change primarily the pace and phasing of projects. Over a longer period it would make it likely that neither will fusion goals be realized nor will plasma physics continue to thrive, at least in the United States. The damage will be felt in many areas of science--applied mathematics, computer science, simulation physics, nonlinear mechanics, classical physics, among others--as well as in the broad area of fusion energy research. This assessment of the effect of level (1986) budgets is the basis for the recommendations of Section 1b.

ii. Fluid Physics

The impact of a level budget for three years could delay needed research and have significant financial consequences on the energy economy. For example, the design of new energy production equipment such as fluidized bed combustors will require an increased understanding of flow in the very complex configurations that characterize such devices. However, given a no-growth budget, the six areas of special importance to DOE should be given priority treatment and the recommendations listed in Section 1b followed.

VI. REFERENCES AND BIBLIOGRAPHY

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APPENDIX A

ERAB PHYSICS REVIEW PANEL
Department of Energy
1000 Independence Avenue SW
Washington, D.C., Room 4A110

Friday, June 13, 1986

8:30 A.M.	Working session of Panel: discussion of charge and objectives
9:00 A.M.	DOE Perspective (Dr. A. Trivelpiece, Director, Office of Energy Research)
9:30 A.M.	DOE organization and budgets (Mr. J. Snow)
9:45 A.M.	Administrative matters (Mr. W.L. Woodard)
10:00 A.M.	Dr. R.L. Kerber, Deputy Under-Secretary of Defense for Research and Advanced Technology
10:30 A.M.	Dr. R.S. Nicholson, Assistant Director for Mathematical and Physical Sciences, NSF
11:00 A.M.	Dr. D.K. Stevens, Associate Director for Basic Energy Sciences, DOE-ER
11:30 A.M.	Dr. W.A. Wallenmeyer, Associate Director for High Energy and Nuclear Physics, DOE-ER
12:00 Noon	Lunch
1:30 P.M.	Dr. J.F. Clarke, Associate Director for Fusion Energy, DOE-ER
2:00 P.M.	Dr. C. DeLisi, Associate Director for Health and Environmental Research, DOE-ER
2:30 P.M.	Discussions of issues raised by the NRC survey (Dr. W.L. Brinkman, Chairman, Physics Survey Committee)
3:00-5:00 P.M.	Working session of Panel: identification of major tasks and assignment of working groups.
5:00 P.M.	Adjourn

ERAB PHYSICS REVIEW PANEL
Sheraton-LaGuardia Hotel
New York City

Monday, August 25, 1986

9:00 A.M.	Review of Charge to the Panel B. Ancker-Johnson
9:15	Information on Basic Energy Sciences funding B.C. Frazer
9:45	Interim subfield reports from working groups:
	a. Condensed Matter H.G. Cohen and D. Chang
	b. Atomic, Molecular, and Optical N. Ramsey and J.R. Huizenga
	c. Plasma and Fluids N. Krall and A. Hansen
	d. Nuclear R. Pry and J. Schiffer
	e. Elementary Particles, Cosmic Rays, Cosmology & Astrophysics J.B. Bjorken and R. Noll
	f. Scientific Interfaces and Technological Applications M. Dresselhaus, D. Chang, and R. Noll
3:00 P.M.	Discussion of general issues and plans for report preparation

ERAB PHYSICS REVIEW PANEL
O'Hare Hilton Hotel
Chicago, Illinois

Tuesday, October 7, 1986

8:30 A.M.	Review of status of panel recommendations B. Ancker-Johnson
8:45	Subfield reports from working groups
3:30 P.M.	Discussion of general issues, conclusions and recommendations
4:00	Discussion of plans for presentation of interim report to ERAB

Wednesday, January 14, 1987

9:00 A.M.	Review of status of the Panel's report B. Ancker-Johnson
9:15	Report on DOE budgets J. Snow
9:30	Detailed review and finalization of the draft report
3:00 P.M.	Discussion of plans for circulation of the draft report and presentation to ERAB at their meeting on February 18-19, 1987

APPENDIX B

MAJOR DOE PHYSICS-RELATED FACILITIES

I. Existing Facilities

Facility	Annual Cost
A. High-Energy Physics	(FY 87)
Alternating Gradient Sychrotron (AGS)	$43M
Tevatron	$131M
Stanford Linear Accelerator Laboratory	$70M
B. Nuclear Physics	(FY 87)
ATLAS	$5M
Tandem/AGS Heavy-Ion Facility	$11M
Bevalac	$22M
88-inch Cyclotron (LBL)	$2M
C.P. Anderson Meson Physics Facility (LAMPF)	$45M
Bates Linear Accelerator Center	$8M
Hollifield Heavy Ion Research Facility	$6M
C. Basic Energy Sciences	(FY 86)
Intense Pulsed Neutron Source (IPNS)	$5M
National Synchrotron Light Source (NSLS)	$18M
High Flux Beam Reactor (HFBR)	$9M
High Flux Isotope Reactor (HFIR)	$14M*
Combustion Research Facility (CRF)	$4M
Los Alamos Neutron Scattering Center (LANSCE)	$4M
Stanford Synchrotron Radiation Laboratory (SSRL)	$9M
D. Fusion Program	(FY 86)
Tokamak Fusion Test Reactor (TFTR)	$73M
Princeton Large Torus (PLT)	$6M
Princeton Beam Experiment (PBX)	n.a. (FY 87 start)
Alcator - C	$8M
TARA	$10M
Advanced Toroidal Facility (ATF)	n.a. (FY 87 start)
International Fusion Superconducting Magnet Test Facility (IFSMTF)	$10M
Doublet III-D (DIII-D)	$29M
Tandem Mirror Experiment Upgrade (TMX-U)	$22M
Mirror Fusion Test Facility (MFTF-B)	not operating
Confinement Physics Research Facility (CPRF)	n.a. (FY 90 start)
Computer Center	$22M

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*Includes Transuranium Processing Plant

II. New Facilities

Facility	Construction Dates Proposed by Laboratories	Estimated Construction Cost (actual year unless stated)	Estimated Operating Cost (FY 86$)
A. High-Energy Physics			
Superconducting Super-Collider	1988-95	$3000M (FY 86$)	$240M
AGS Accumulator/Booster	1986-89	$28M	$1M**
B. Nuclear Physics			
CEBAF	1987-93	$255M	$31M
Relativistic Heavy-Ion Collider	1989-92	$240M (FY 86$)	$38M*
C. Basic Energy Sciences			
Advanced Light Source: 1-2 GeV	1987-93	$100M	$19M(FY 92)
Advanced Photon Source: 6-7 GeV	1988-93	$385M	$48M (FY94)
Advanced Neutron Source	About 1991-97	About $300M*	$15-20M**
D. Fusion Program			
Compact Ignition Tokamak	1988-93	$412	$73M

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*Rough estimate based on design approaches considered most probable

**Incremental costs supplementing or replacing existing facilities

APPENDIX C 1/87

DOE PHYSICS FUNDING (Operating budgets in millions)

1. Office of Energy Research

	FY85	FY86	FY87
High Energy Physics	$ 369.2	365.4	398.3
Nuclear Physics	142.3	142.7	166.7
Basic Energy Sciences*	117.4	119.1	137.4
Magnetic Fusion	369.6	320.5	309.1
Subtotal, OER	$998.5	$947.7	$1,011.5

2. Other DOE

			(1)
Nuclear Energy	0.5	0.0	0.0
Fossil Energy	6.7	6.2	2.9
Defense Energy	186.5	168.6	173.4
Total DOE	$1192.2	$1,122.5	$1187.8

(1) Requested by DOE. Congressional action changed funding, but new numbers are not available.

*Basic Energy Sciences is assumed to be 50% physics research.

APPENDIX D

10/86

NUMBER OF PHYSICS DOCTORATES BY SUBFIELD, 1969-1985

Subfield	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985
Solid State	360	402	442	393	400	351	319	282	257	243	243	202	250	235	221	258	248
Elementary Part.	220	258	278	198	222	148	124	128	138	135	119	117	117	118	136	138	154
Nuclear (1)	188	212	227	234	182	144	130	96	93	77	103	73	62	53	90	72	86
Atomic & Molec.	127	152	124	150	122	120	138	116	105	88	72	69	65	96	71	77	58
Plasmas	61	85	86	93	74	57	53	75	72	68	62	59	65	69	72	73	55
Astrophysics	55	63	54	66	67	77	71	72	57	74	57	69	59	50	65	56	57
Optics	16	30	25	31	33	26	33	50	31	33	46	43	54	42	50	53	50
Acoustics	11	22	20	20	15	11	12	9	12	14	13	23	13	11	14	21	10
Fluids	24	21	20	27	28	22	22	20	14	13	14	15	14	13	15	11	16
Other	345	364	405	360	383	322	331	309	307	257	321	263	266	274	258	279	301

SOURCES: NAS and American Institute of Physics

(1) Between 1969 and 1982, this category was listed as Nuclear Structure. In 1983, it was changed to Nuclear Physics which it was felt more accurately described the scope of the subfield. This change to a broader definition is probably responsible for the apparent upswing in degree production in this subfield between 1982 and 1983. The earlier dramatic decline in degree production in the late 1970' and early 1980's reflected only degrees awarded in nuclear structure, an underestimate of the total number produced in nuclear physics.

Senator **Ford**. Thank you very much, doctor.

I think that last part of your statement there was kind of an add-on, reading your previous or the statement submitted earlier. So you have become very emphatic about your position as it relates to the SSC, am I correct in that, to at least delay it for a while?

Dr. **Ancker-Johnson**. I'm saying that it would be in the best interests of the program itself to assess whether there is a prospect for using superconducting material that has that property at higher temperatures, because one would not want to build a machine which operated at a more expensive level than needed or that in fact could have been improved had these better materials been used.

Senator **Ford**. This is kind of a phenomenon that has kind of come on in a short period of time, and everybody is kind of hyped about it, I think. And you are telling us to get both feet back on the ground and look at it, not through rose-colored glasses, but through reality, and not jump too fast?

Dr. **Ancker-Johnson**. I think I am saying, sir, that superconductivity has made such enormous strides in the recent past that it would be a great mistake to go forward with the—in my personal opinion, with the SSC design, putting it into concrete, as it were, before an assessment were made as to how the impact of these discoveries might indeed change the design and make it a less expensive machine to operate, and possibly even a better machine.

Senator **Ford**. What would we lose if the SSC were delayed until say the federal budget was in better shape and more research had been done? What do we stand to lose if we do not proceed forthwith, posthaste, or however?

Dr. **Ancker-Johnson**. I'm sure, Mr. Chairman, that those who are following after me would love to answer that question, and would do so with great zest. Trying to represent the whole of physics, I would say that a delay would not be a great harm, particularly because if there were a synergy between the development of the SSC and the furthering of commercial application of the recent superconducting discoveries, it would be a very great benefit both to the high energy community and to all of physics and all those commercial applications, meaning it would be of great advantage to the whole country.

Senator **Ford**. I had a question for you that I think you have already answered. I had a question, would you support the funding of the SSC if the result was a reduction in the other programs for basic science? And I am sure your answer now is no, so I will not—am I correct in that?

Dr. **Ancker-Johnson**. Yes, indeed you are, Mr. Chairman. That is really the whole thrust of our report, is that one field of physics should not overweigh all the others. And indeed, if we are looking at tight budgetary times, the so-called small physics fields, condensed matter physics and atomic, molecular, and optical physics, are really much more applicable to our economic situation, to commercial invention, and that sort of thing.

And so it would be extremely unwise to lower the support for those fields.

Senator FORD. I think you have answered the questions I had that would be important for me, and I will not pursue any other questions.

Senator Melcher.

Senator MELCHER. Thank you, Mr. Chairman.

Doctor, what is the cost of the other programs that you say must not be sacrificed?

Dr. ANCKER-JOHNSON. I am just checking if our report—let's see. The current estimates in 1986 dollars for the Compact Ignition Tokomak is $412 million; for the 6 to 7 GeV Advanced Photon Source, $385 million.

Senator MELCHER. Are those federal dollars?

Dr. ANCKER-JOHNSON. Yes, sir.

For the Relativistic Heavy Ion Collider, $240 million. And for the advanced—no, let's see here.

About $300 million. Apparently there was not a better estimate at that time. At this time, that's the furthest out in time for construction start. But it's about $300 million.

Senator MELCHER. Those are not all programmed, though, in fiscal 1988?

Dr. ANCKER-JOHNSON. No.

Senator MELCHER. Those are not fiscal 1988. Those are total dollars?

Dr. ANCKER-JOHNSON. Yes, and they're staggered over quite a few years, beginning with 1988 and going through 1997.

Senator MELCHER. Yes, for about 10 years.

Dr. ANCKER-JOHNSON. Yes, sir.

Senator MELCHER. And that adds up to about a billion dollars?

Dr. ANCKER-JOHNSON. In excess of a billion, yes.

Senator MELCHER. About $1.2 or $1.3.

Dr. ANCKER-JOHNSON. $1.4 or so.

Senator MELCHER. $1.4?

Dr. ANCKER-JOHNSON. Yes, sir.

Senator MELCHER. And so you are saying that, unless they can be funded as programmed over the next 10 years, for SSC then you would recommend withholding. But the Chairman's questions concluded that, even if we could fund both, you would still recommend holding off a little bit?

Dr. ANCKER-JOHNSON. I am only suggesting, not that funding be stopped—there is funding currently for this sort of work I am suggesting, namely an assessment of the possible impact of these new discoveries. I would not suggest that the effort be stalled at all. I think that would be a mistake.

But we also should not rush on with technology that might be outmoded. And I of course would be very excited if there turned out to be some synergy that could advance both this highly basic field of physics and commercial applications, and certainly the making of magnets for the high energy physics facility could be a wonderful synergism.

Senator MELCHER. In other words, proceed on schedule with both?

Dr. ANCKER-JOHNSON. Yes. I would think it might be that if there is a considerable impact of the new discoveries on the design, that it might delay the start of construction by some period of time. But that would only be apparent after the assessment were complete or at least well under way.

Senator MELCHER. Thank you very much, doctor.

Senator FORD. Thank you, Senator Melcher.

Doctor, there may be a question or two that I would like to submit to you in writing, and I would ask that you respond accordingly.

We thank you for your patience.

Dr. ANCKER-JOHNSON. Thank you.

Senator FORD. We are going to make it. Thank you very much, doctor.

Next we will have a central design group panel. It will be Dr. Steven Weinberg, Nobel Laureate in Physics in 1979, and Dr. Maury Tigner, Director of the Central Design Group, Superconducting Super Collider.

If you two gentlemen will come forward, we will proceed. Dr. Weinberg, if you would like to go first, and if you would like to submit your total statement in the record and highlight it, we would appreciate it.

STATEMENT OF DR. STEVEN WEINBERG, THEORY GROUP, PHYSICS DEPARTMENT, THE UNIVERSITY OF TEXAS AT AUSTIN

Dr. WEINBERG. Thank you, Mr. Chairman.

I have submitted a slightly longer statement. I am going to try to give a brief summary of it.

I'm grateful to you and the members of the Committee for the chance to come here today and to say why I think it's so important to go ahead with the SSC. You have heard and I think you will hear a number of valid and important reasons to build the SSC. I am going to concentrate here on just one of them, but it is one benefit of the SSC that underlies all the others.

That is that it will allow us to make great scientific discoveries. Perhaps I should begin by explaining that I am not one of the experimental physicists who are designing the SSC, like Dr. Tigner, or who would use it to do experiments when it's finished.

I am a theorist. I am one of those who hope to use the data that is provided by the accelerator as a check on existing theories of elementary particles or as clues to new theories. So I suppose you could say that I'm here as a consumer representative.

It's on this basis that I want to tell you that we theorists are desperate for the SSC to be built. You see, we're stuck. In this century, we've seen an impressive progress toward understanding matter and the forces that act on it in more and more comprehensive and unified terms. By the early 1970's, we had a pretty satisfactory theory, known as the standard model, that accounted for all the particles and forces that you find in naturally occurring atoms and molecules, or that could be produced with existing accelerators.

It certainly wasn't a final theory. In many respects it was slapped together, had arbitrary elements, holes, and loose ends. It left out the existence of gravity. It was clearly just an approximation, a low energy approximation to perhaps a final theory.

Well, ever since the early 1970's, when the standard model was completed, the experimentalists have done a brilliant job of confirming the outlines of the standard model and filling in some of the holes. But no clues have turned up experimentally with the accelerators that are now available that would help us to take the next step beyond the standard model.

Theorists like myself and many others have tried to do it on our own. We have developed ideas that are more and more ingenious and mathematical and sophisticated. Some of these ideas are probably part of the truth, but after more than a decade we still have no clear and definite idea of anything beyond the standard model.

Just as they used to tell us in high school, physicss really is an experimental science.

I am quite convinced that the SSC is just what we need to get us moving again toward a final theory. It would increase the energy with which we can study particle collisions by twentyfold, and that means a roughly tenfold increase in the masses of the particles that we can produce and study in the laboratory.

Now, every time that we increase the energies available in our accelerators by an appreciable amount, accelerator physicists have always been lucky; they have always found exciting new phenomena which suggest exciting new theories.

However, in the case of the SSC we can say more than that. In the case of the SSC, we can say that we can be really quite sure that at least one scientific discovery of the greatest importance would be made.

It has to do in part with the relations between two of the fundamental forces of nature. One of them is the force of electromagnetism, which is familiar to everyone in its manifestation in electric currents, magnetic fields, light itself.

The other is much less familiar. It's called the weak nuclear force. It plays an important role in radioactivity and it plays an essential role in the chain of nuclear reactions that gives the energy to the sun.

One of the thing we learned in working out the standard model is that there's a symmetry between these two forces, and by that I mean that in the equations of the theory these forces appear almost interchangeable. In the level of the underlying equations, you can hardly tell whether you're talking about electromagnetism or weak nuclear forces.

But the symmetry is certainly not apparent in the laboratory. There are obvious differences between electromagnetism and radioactivity. The symmetry is broken. We think the symmetry was broken as the universe cooled, about a trillionth of a second after the big bang.

Now, this may all seem very esoteric, but in fact if the symmetry were not broken then all the elementary particles that we know would be massless. That is, they would all travel at the speed of light, it would be impossible for ordinary matter to hold together, there would be no atoms or molecules or scientists or Senators.

Senator FORD. In that order? [Laughter.]

Dr. WEINBERG. Well, I would almost. [Laughter.]

Senator FORD. I am sorry to break your train of thought.

Dr. WEINBERG. I was tempted for a moment to say, in order of increasing mass, but I realized that that's a disastrous remark. [Laughter.]

Senator FORD. How about mass of paperwork.

Dr. WEINBERG. Thank you, yes.

So it's a matter of great importance. The biggest hole in the present standard model worked out by the early 1970's is that we're not sure of the mechanism by which this symmetry breaking occurs.

But one thing is clear. It must involve new forces, beyond any of the forces—electromagnetic or nuclear forces or gravitation—about which we already know. According to the simplest picture of these new forces, they involve a new class of particles so far undiscovered, called Higgs bosons. But even though we've given them a name, we don't really know their detailed properties.

We don't know how many types there are. We're not even really sure that Higgs bosons exist as elementary particles. But whatever they are, whatever these new forces are, whether or not they involve elementary Higgs bosons, we have firm mathematical theorems that show that some trace of these new forces are going to show up at the energies and luminosities that will be available in the SSC.

And in fact, the question keeps coming up: Why 20 TeV? This is a large part of the answer. The SSC is designed so that we will be sure that it will settle at least this question, the question of the breakdown of the symmetry between the weak and the electromagnetic interactions. In other words, the SSC is a guaranteed no-lose proposition.

In saying this, I think of Columbus promising the King and Queen of Spain that if he sailed west far enough he would reach the Indies. Now, of course that wasn't right. What he could have said if he had been more careful, what would have been correct, is that if he sailed west far enough from Spain he would reach the Indies unless something equally interesting got in the way. Of course, America got in the way.

For the SSC, we can say that the SSC will discover the Higgs boson unless something equally interesting gets in the way.

Of course, this is not the only thing we're going to learn from the SSC. There's a long list of questions waiting to be answered about hypothetical new particles or forces or symmetries of various kinds. And if past experience is any guide, the most exciting—well, some of the most exciting things learned at the SSC will be things that no one has thought of yet.

But it's nice to know that there's at least one important question that can be counted on. And as far as I know, there's simply no disagreement in the United States high energy physics community that the SSC is the machine to build next.

But so what? Who cares about these discoveries? Higgs bosons, weak nuclear forces, it's all very removed from everyday life. It's even removed from everyday economic life.

Why should our fellow citizens care about this? I think the ultimate justification of the effort that goes into elementary particle physics and of the cost of facilities like the SSC that we need to carry it out is that we're following a historic path toward physical principles that explain more and more about nature, on a deeper and deeper level.

We've been learning how to explain—well, in the early part of the century we learned how to explain the ordinary properties of matter in terms of atoms and radiation. And then we learned how to explain those in terms of electrons and protons and so on. And then we learned how to explain those in terms of the deeper field theory that I've been calling the standard model.

And now we want to take the next step in this historic process. Our historical experience teaches us that by working at the most fundamental level, the work—the discoveries we make can be counted on to have great impact on other areas of physical science, and it inevitably generates new inventions and technologies that change our lives, although it's not always easy to tell in advance what they'll be.

But I would say in elementary particle physics in particular, in the kind of elementary particle physics that would be done at the SSC, the kind of phenomena that we're studying are so far removed from everyday life, the conditions of energy and temperature and density are so extreme and extraordinary, that the work itself evokes a kind of athleticism of technology.

It pushes the art of computing and vacuum technology and cryogenics and superconductivity and all that to its limits. It's hard to calculate these things, and I'm not an expert on technological spinoffs. But you can just look at what our economic competitors are doing.

The Japanese, after starting with nothing but a theoretical establishment, people like me, in the early sixties realized that their science was not balanced, that they had to develop experimental high energy physics. And they are just now beginning work on what is righ now one of the world's leading high energy facilities, the TRISTAN accelerator.

The Russians are continuing to develop an accelerator, the UNK accelerator, that, if we don't build the SSC, for a while will be the leading accelerator in the world. In addition to—well, I presume they have done the calculations and have concluded that this kind of science is important to their technology. And in fact, evidence of that is the fact that companies like Mitsubishi and Toshiba are actively supporting, even at a loss to themselves, the work that's being done in Japan in high energy physics.

This kind of work also promotes an athleticism of the intellect. It forces us to deal with concepts that are more and more abstract and sophisticated. And these concepts very frequently turn out to have important applications in fields like solid state physics.

I was testifying in the other body this morning and I sat next to Robert Shrieffer, who is one of the world's leaders in the field of superconductivity. And he testified that his own work in superconductivity was partly inspired by developments in elementary particle physics. And I must say, in fairness to Dr. Shrieffer, that the flow sometimes goes the other direction also.

However—and now I think I'm near my end—it may be that for our fellow citizens in general and not just for physicists, these applications and spinoffs don't really capture the most important aspect of elementary particle physics.

We spend—I do, we all do—spend most of our lives doing our jobs and taking care of our families. But I don't know of anyone who doesn't occasionally go out at night and look at the sky and wonder what the universe is all about, what are the rules, why is it the way it is.

There are reasons to believe that as we learn more about elementary particles we're learning something quite deep about the logical structure of the universe. And this is the reason I say it. At the same time that we've been moving toward higher and higher energies and smaller and smaller structures, we find that our theories have moved steadily toward greater and greater simplicity.

I don't mean that our list of supposed elementary particles always gets shorter, and I certainly don't mean that the mathematics gets easier. What I mean is that the physical principles, the rules of the game that we discover, have become more and more coherent and universal as we proceed.

And we're beginning to feel confident that this simplicity that we've been discovering is not just an accident of the particular kinds of problems we've chosen to study or the particular moment in the history of science that we've reached, but that this mirrors the simplicity and the beauty that's built into the universe at its deepest levels. I frankly think that this discovery is one of the things going on in our country and in our present civilization at which men and women of the future, and not just physicists, are going to look back at with respect.

Thank you.

[The prepared statement of Dr. Weinberg follows:]

Testimony before the Subcommittee on Energy Research and Development of the U.S. Senate Committee on Energy and Natural Resources, April 7, 1987

Steven Weinberg
Theory Group, Physics Department
The University of Texas at Austin
Austin, Texas 78712

Mr. Chairman and members of the Committee: I am grateful to you for giving me the chance to come here today, and to say why I think is it so important to go ahead with the superconducting supercollider (the "SSC").

You have heard or will hear a number of different reasons why we should build the SSC. There are spin-offs from the new technologies that will have to be developed to build it. It is a symbol of America's commitment to science. It will keep up the momentum of American science, and save us from losing a whole generation of high energy physicists to laboratories abroad. It will do great things for the economy of the state where it is sited. All these reasons are valid and important, but I will speak here mostly about just one benefit of the SSC, one that underlies all the others: it will allow us to make great scientific discoveries.

I think I should begin by explaining the perspective from which I'm viewing this issue. I am not one of the experimental physicists who are designing the accelerator, or who would use it to do experiments when it is finished. I am a theorist, one of those who hope to see the data provided by the accelerator used to confirm or reject existing theories, or to suggest new theories about the elementary particles. So I suppose you could say that I'm here as a consumer representative.

It is on this basis that I want to say that we theorists are desperate for the SSC to be built. You see, we are stuck. This century has witnessed an impressive progress toward understanding matter and the forces that act on it in more and more comprehensive and unified terms. By the early 1970's, we had a pretty satisfactory theory, known as the "standard model", that accounted for all the particles and forces that could be observed in naturally occurring atoms or molecules, or that could be produced with existing accelerators. But it certainly wasn't a final theory — it was full of holes and loose ends, and left out little details, like the existence of gravity. Since the early 1970's the experimentalists have done a brilliant job of confirming the outlines of the standard model and filling in some of its holes, but they haven't been able to find any clues that could help us to take the next step beyond the standard model. We theorists have tried to do it on our own. Our theories have become more and more ingenious and sophisticated. Some of them, like the superstring theories, are probably part of the truth. But after more than a decade, we still have no clear and definite idea of anything beyond the standard model. Just as they used to tell us in high school, physics really is an experimental science.

I am quite convinced that the SSC is just what we need to get us moving again toward a final theory. It will increase the energy at which we can study elementary particle collisions by 20-fold, with a corresponding roughly 10-fold increase in the mass of the elementary particles that we can create and study.

Of course, every time we make a sizable increase in the energy of our accelerators, we open up the possibility of discovering important new physics. Generally each new generation of accelerators has been pretty lucky at finding something exciting. But in the case of the SSC, we can be really quite confident of making one particular new discovery of the greatest importance.

I don't supprose you want me to give a physics lecture here, but I would like to take a moment to explain this one point. One of the things we learned in working out the standard model is that the equations that describe the forces that act on elementary particles are much simpler than you would expect from the way the forces behave in our laboratories. In particular, two of the forces — the familiar one known as electromagnetism and a less familiar "weak nuclear force" that plays a key role in radioactivity and in the production of energy by the sun -- appear almost interchangeable in our equations. We say that there is a symmetry between these forces. But this symmetry is certainly not apparent when you study electromagnetism or radioactivity in the laboratory -- the symmetry is broken. It's much like what happens when water freezes. The equations that govern the molecules of water certainly don't make distinctions between different directions in space -- we say that in these equations, there is a symmetry among all spatial directions. This symmetry is apparent in a glass of liquid water -- if you sit on one molecule, the chance of finding another molecule within a certain distance in one direction is the same as in any other direction. But when the water freezes, the symmetry is broken -- the water molecules form a regular pattern of rows and planes stretching off in certain special directions. We believe that in much the same way, the symmetry between the weak nuclear and the electromagnetic interactions was broken in the early universe, about a trillionth of a second after the big bang, when the temperature of the universe had dropped low enough, to about ten thousand trillion degrees, for this kind of freezing to occur.

The biggest hole in the standard model is that we are not sure of the mechanism by which this symmetry breaking occurs. It is clear however that it involves new kinds of forces, beyond the electromagnetic and nuclear forces, but there are several possibilities. The simplest of these involves one or more species of new particles, the so-called Higgs bosons. No one has ever detected Higgs bosons, which presumably means either that they don't exist, or they are too heavy to have been produced yet. But our theories tell us that the Higgs bosons can't be heavier than about a trillion electron volts (TeV) . The SSC is designed so that if Higgs bosons exist, they will be found.

Even if Higgs bosons don't exist, there must be some sort of new force responsible for breaking the symmetry between the electromagnetic and weak nuclear forces, and there are mathematical theorems that show that some trace of these new forces must show up at the energies that will be available in the SSC. Roughly speaking, this is because if there were no new forces we could extrapolate our existing theories upwards in energy up to the point were they would give nonsensical results, like probabilities greater than 100%.

In other words, the SSC is a guaranteed no-lose proposition. (The phrase "no-lose" proposition is borrowed from the title of a paper presented by Dr. M. Chanowitz at the international conference on high energy physics at Berkeley last summer.) It is a little like Columbus sailing west from Spain. He said he was going to the Indies, but if he had been more careful, he might have said that by sailing far enough west he was sure to get to the Indies unless something else equally interesting got in the way. As it happened, of course, a whole new world was in the way.

I would not be able to speak so confidently if the energy of the SSC were reduced. For example, high energy physicists in Europe have been considering the possibility of putting a supercollider in an existing tunnel, which would limit the energy in each beam to 7 to 9 TeV, compared with the SSC's 20 TeV. A supercollider with an energy of, say, 10 TeV might perhaps be able to find the Higgs boson, but it would not be able to explore its properties very far. It would in effect be serving as a scout for the SSC. On the other hand, if the Higgs boson has a mass above about 1/2 TeV, which it well might have, it probably couldn't be found at all at a 10 TeV supercollider. (It is not just a question of having enough energy available to produce the Higgs bosons; one must produce them in sufficient numbers to show up clearly.) Also, if there is no

Higgs boson, a 10 TeV machine would almost certainly not be able to observe the other new forces that would take its place. Lowering the energy of the SSC would save some money, it would save very little time or scientific effort, and it would be very risky for the future of particle physics. As far as I know, within the U.S. high energy physics community there is simply no disagreement that the SSC is the machine to build next.

This question of symmetry breaking is crucially important to us, both because the deepest information we have about the laws of physics concerns their symmetries, and also because its breakdown necessarily involves a new class of particles and forces, beyond anything we have yet observed. But this is not the only thing we will learn from the SSC. There is a long list of questions waiting to be answered:

1) Are there more species of heavy quarks and leptons? (Quarks are particles similar to those out of which protons and neutrons are composed; protons and neutrons are the particles out of which atomic nuclei are composed; and leptons are particles similar to the electrons that make up the outer parts of ordinary atoms.)

2) Are these quarks and leptons, that we call elementary particles, themselves made up of more elementary constituents?

3) Are there new types of weak and/or strong nuclear forces?

4) Do all these particles have counterparts with different spin, with properties related by the hypothetical new symmetry called supersymmetry?

And if past experience is any guide, some of the most exciting discoveries made at the SSC will be things that no one has dreamed of yet. But it's nice to know that there is at least one major discovery that can be counted on.

But who cares about these discoveries? After all, we don't encounter temperatures of ten thousand trillion degrees very often in everyday life. It isn't that we just want to slam protons into each other at 20 trillion volt energies, like a demolition derby, because it hasn't been done before. It isn't even that we want to add a few more entries to the list of elementary particles. I think that the ultimate justification of the effort that goes into elementary particle physics, and of the cost of the facilities like the SSC that we need to carry it out, is that we are following a historic path, toward physical principles that explain more and more, on a deeper and deeper level.

We learned in the early part of this century how to explain all the ordinary properties of matter and forces — why is the sky blue?; why is iron hard?; why is water transparent? — in terms of the properties of atoms and their interaction with radiation. We then learned how to explain these properties in terms of the particles out of which atoms and radiation are comprised — electrons, neutrons, protons, and photons. More recently, we learned how to explain these properties in terms of a deeper field theory, the "standard model". And now we want to take the next step, for which we need the SSC.

If history is any guide, physics research at the most fundamental level can be counted on to have great impact on other areas of physical science, and it eventually generates new inventions and technologies that change our lives. Also, in elementary particle physics we are working at a level of reality so far removed from everyday experience, that the work evokes a kind of athleticism of technology, pushing the art of computing and cryogenics and vacuum pumps and all that to its limits, and also an athleticism of the intellect, forcing us to deal with concepts that are more and more abstract and sophisticated, concepts that then frequently turn out to have important applications in fields like solid state physics or hydrodynamics.

But it may be that for our fellow citizens in general, and not just for physicists, these applications and spin-offs do not capture the most important aspect of elementary particle physics. Who is there that never wonders about what sort of universe this is? There are reasons to believe that as we learn more about elementary particles, we are learning something quite deep about the logical structure of the universe.

You see, at the same time that we have been moving toward higher and higher energies and smaller and smaller structures, our theories have moved toward greater and greater simplicity. It is not that our list of supposed elementary particles gets steadily shorter, or that the mathematics gets easier; it is that the physical principles, the rules of the game, have become more and more coherent and universal. We begin to suspect that this simplicity that we have been discovering is not just an accidental feature of the present state of our theories, but mirrors a simplicity, a beauty, that is built into the universe at its deepest levels.

Senator **Ford**. Thank you, doctor.
Dr. Tigner.

STATEMENT OF DR. MAURY TIGNER, DIRECTOR, CENTRAL DESIGN GROUP, SUPERCONDUCTING SUPER COLLIDER

Dr. **Tigner**. Thank you, Mr. Chairman, for this opportunity to appear before you with an account of our activities.

If approved and constructed, the SSC will provide scientists a unique instrument of unprecedented power, as you just heard described. Its 20 trillion electron volt beam energy will permit them to strike deep into new territory, where we can be confident of totally new perspectives on the fundamental structure of the physical world.

This drive is today's manifestation of the centuries old striving to understand, in our terms, what are the basic building blocks of matter, and what are the rules by which these building blocks are put together to form the world around us.

The pursuit of that quest has led to our current understanding of the hierarchical structure of matter: macroscopic stuff made of molecules, molecules of atoms, atoms of electrons and nuclei, nuclei of protons and neutrons, protons and neutrons of quarks, and so on.

From this understanding and the concomitant comprehension of the forces responsible for holding these constituents together has flowed the enormous wealth in knowledge, culture, and economic activity that we possess today, and which touches our daily lives in countless ways. The SSC will empower us to drive forward this knowledge foundation building process which has been so successful.

In addition to this scientific program, of which you have heard particulars from Dr. Weinberg, there will be immediate benefits from the creating of the SSC and the scientific detector and information processing equipment that will go with it.

We will be challenging technologists and industry to provide the latest in technological ideas, materials, and devices, thereby strengthening their high technology muscle and competitive position.

The proposed scientific program for the SSC and its technical realization have been meticulously thought out. Already in the 1970's, the need for an accelerator having the scientific capabilities now incorporated in the SSC design was being widely discussed.

In 1984, the DOE charged the Universities Research Association, a consortium of 56 universities engaged in high energy physics research, to coordinate and direct preliminary R&D to prepare a conceptual design for this SSC, a detailed cost estimate, a study of siting requirements, and an example plan and schedule by which the SSC might be realized.

Personnel for this effort are drawn largely from the universities and DOE laboratories, although significant help has been received from individuals seconded from laboratories abroad. Persons from 22 institutions are involved.

We have been hard at work on our assignment ever since. The conceptual design and cost estimate that have emerged are captured in a five volume conceptual design report, shown to you here by Dr. Trivelpiece, and it's available from the DOE.*

The scientific, technological, and cost estimates are treated therein. The design is shown schematically at figure one and figure two of my testimony which I have submitted to you, figure two being a plan view accompanied by a collage representing a wide variety of important technical and civil engineering components.

This design, in which counter streaming beams of protons at an energy of 20 trillion electron volts each are caused to recirculate one above another by a superconducting magnetic confinement system and made to collide at six designated places, follows the now well established cascaded synchrotron collider concept.

Several installations employing this general scheme, albeit on a smaller scale, have operated successfully. Thus, the needed systems and processes are known from past experience and much expertise has been created upon which we could draw for the conceptual design and cost estimates, and we have done that.

The conceptual designs of each technical system, sub-system, and major component were made by recognized experts, as were cost estimates for these components. In important cases, detailed concept designs for components, such as magnets, were taken from working models to assure accuracy of detail.

These permitted example manufacturing plans to be made by manufacturing companies for the purpose of detailed estimating of assembly as well as sub-component costs. These cost estimates were checked in detail by independent experts.

The principal conventional systems of the SSC are the 53 mile underground tunnel and the collision halls. Accordingly, an extensive study of these systems was made as part of the conceptual design and cost estimate. In view of the unknown location of the eventual site, a broad survey of possibilities was undertaken.

Characterizations of the topologial and geological distributions to be found within the United States were studied. It was found that by making three quite different conceptual designs for the civil works, examples covering most of the United States were in hand.

In working out the examples, the particular types of construction difficulties that inhere to the three different types of site were taken into consideration. Detailed simulation models for construction in these various circumstances were put together and tested on actual jobs that had been carried out to verify the accuracy of their predictions.

The conclusions were that construction costs for the three widely different types of sites are quite similar. The cost differences are likely to be dominated by individual peculiarities at actual sites, rather than their generic type.

Thus, we can be sure that there are many satisfactory sites across the United States. Prevailing local details will have to be used to sort them out.

In the area of technical systems, over 6,000 individual items were considered in the cost estimate, including the needed engineering, supervision, and contingency. These are reported in the 900 page volume D of the conceptual design report.

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*SSC-SR-2020, Superconducting Super Collider, March 1986.

In the area of conventional systems, buildings, tunnels, utilities, and the like, 3,500 elements were considered. They are reported in some 300 pages of volume C, an appendix to this conceptual design report.

The results of the cost estimate are reported in table 6 and have been widely known, and include all things necessary to construct the SSC. The total, exclusive of costs of obtaining the site, is $3.2 billion. When we put together all of the other things that it will be necessary to provide to actually start doing research on the machine, the grand total in Fiscal Year 1988 dollars comes to $4.375 billion.

In addition, the annual operating cost of the facility has been estimated from past experience, and includes the operating and service personnel, materials and supplies, utilities, accelerator and detector equipment, and anticipated accelerator and plant improvements. And the estimate comes to $270 million annually.

The conceptual design and its accompanying cost and schedule estimates have been extensively reviewed by the DOE and the concerned technical community. They are found to be thorough and credible.

It is appropriate to observe that the SSC is a project in one field of physics, but that it resides in the broader context of research of all kinds and can benefit from developments in other areas, as our science has many times in the past.

There has been tremendous excitement in recent months in the field of superconducting materials because of the discovery of the rare earth copper oxides that remain superconducting to much higher temperatures than achieved before.

The applications of higher temperature superconductors have been much discussed even before the latest developments. There certainly is a potential for revolutionary changes in many areas of technology. Large particle accelerators like the SSC may well also benefit, although they have very special needs.

The history over the past 20 years of the development of the present multifilamentary, high current capacity superconductor suitable for fabrication of accelerator magnets suggests the need for considerable R&D effort to get from the present exciting developments to the thousands of miles of useful strand for a project like the SSC.

If, as I would guess, the realization of useful material for accelerator magnets takes a considerable number of years, the work of the SSC could benefit at some future time through a significant enhancement in its research power.

The technical feasibility and cost of the SSC have been established. As a result of the underlying R&D and conceptual design studies that have been accomplished, a strong foundation for beginning detailed engineering design and development and for expanding the already considerable industrial involvement has been laid. Thus, if approved, the SSC is ready to move forward.

In closing, I'd like to quote from a recent speech to the Materials Research Society by Dr. George Pake, then Group Vice President of the Xerox Corporation. In referring to the SSC, he said:

Why do I favor the major investment, this gargantuan particle physicists' "boondoggle" or whatever worse name its critics may give it? How can a hard-bitten industrialist, with all his pragmatic concerns, be an outspoken advocate of such a large and costly project, that has so little direct economically tangible return?

It's simple. For the near term and long range future vigor and benefit of the U.S. industrial and even military economy, the most pragmatic posture I can imagine is one that opts by overt choice for the most advanced knowledge and deepest understanding.

For the United States instead to deliberately choose a "me-too" or "also-ran" research science status is to opt for ignorance rather than knowledge. Nothing is more pragmatic than the broadest and deepest knowledge base mankind can attain.

And I am just chauvinistic enough to believe that the welfare and economic vigor of the United States is best assured if we lead the world in advancing knowledge and in developing that broad, deep knowledge base.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Tigner follows:]

STATEMENT OF MAURY TIGNER

SSC CENTRAL DESIGN GROUP
UNIVERSITIES RESEARCH ASSOCIATION

before the

SENATE SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Mr. Chairman, members of the Subcommittee, thank you for this opportunity to appear before you with an account of SSC activities.

If approved and constructed the SSC will provide scientists a unique instrument of unprecedented power. Its 20 trillion electron volt beam energy will permit them to strike deep into new territory where we can be confident of totally new perspectives on the fundamental structure of the physical world. This drive is today's manifestation of the centuries-old striving to understand, in our terms, what are the basic building blocks of matter and what are the rules by which these building blocks are put together to form the world around us. Pursuit of that quest has led to our current understanding of the heirarchical structure of matter: macroscopic stuff made of molecules, molecules of atoms, atoms of electrons and nuclei, nuclei of protons and neutrons, protons and neutrons of quarks. From this understanding and the concommitant comprehension of the forces responsible for holding these constituents together has flowed the enormous wealth in knowledge, culture and economic activity that we possess today and which touches our daily lives in countless ways. The SSC will empower us to drive forward this knowledge foundation building process which has been so successful.

In addition to this scientific program, of which you have heard particulars from Dr. Weinberg, there will be immediate benefits from the creating of the SSC and the scientific detector and information processing equipment that will go with it. We will be challenging technologists and industry to provide the latest in technological ideas, materials and devices thereby strengthening their high technology muscle and competitive position.

The proposed SSC scientific program and its technical realization have been meticulously thought out. Already in the 1970's the need for an accelerator having the scientific capabilities now incorporated in the SSC design was being widely discussed. In 1978 and 1979 two international workshops were held under the auspices of the International Committee on Future Accelerators (ICFA). Various possible approaches to technological realization of the scientific requirements were considered there by the international community of experts in the field. In 1982 an intense review of scientific need and practical technological approaches was carried out in the U.S. under the auspices of the Division of Particles and Fields of the American Physical Society. From these studies and similar activities in 1984 and 1986 have emerged a broad consensus concerning the need and most feasible technological approach for addressing the science. Following specialized technical studies in late 1982 and early 1983, the High Energy Physics Advisory Panel (HEPAP) to the Department of Energy unanimously recommended to the DOE the initiation of a high luminosity proton-proton collider-accelerator project, now known by the acronym, SSC.

In response, the DOE and the Directors of the major U.S. accelerator laboratories commissioned a Reference Designs Study in early 1984 to explore more fully the technical feasibility and cost of the SSC. This study examined three approaches to high luminosity proton-proton colliders. The study, which was intensely reviewed by the DOE and the concerned community, concluded that the SSC is technically feasible and could be built for a sum of three billion dollars including considerable contingency. The study also outlined R&D and further studies needed to check the assumptions made in the Reference Designs Study and to select among the various possible approaches it treated.

Later in 1984 the DOE charged the Universities Research Association (URA), a consortium of 56 universities engaged in high energy physics research, to coordinate and direct that preliminary R&D, to prepare a conceptual design of the SSC, a detailed cost estimate, a study of siting requirements, and an example plan and schedule by which the SSC might be realized. In carrying out this charge, a special Board of Overseers was established by URA for the purpose. Candidates for the Board were selected by a committee of University Presidents from a list of nominees put forward as a result of a nationwide search. The Board is constituted of senior representatives from U.S. industry, academic and DOE laboratory communities. The Board established a Central Design Group(CDG) located on the premises of Lawrence Berkeley Laboratory. The hospitality of LBL has been generous and is very much appreciated. Personnel for the CDG are largely drawn from universities and DOE laboratories, although significant help has been received from individuals seconded from laboratories abroad. R&D work outside the CDG has taken place primarily at Brookhaven National Laboratory, Fermi National Accelerator Laboratory, Lawrence Berkeley Laboratory, Texas Accelerator Center, University of Wisconsin and industrial firms. Table 1 shows the roster of university and laboratory institutions

Table 1

Institutional Participation in the SSC R&D and Conceptual Design Report

Brookhaven National Laboratory
Fermi National Accelerator Laboratory
Lawrence Berkeley Laboratory
Los Alamos National Laboratory
Stanford Linear Accelerator Center
Texas Accelerator Center

University of California, Berkeley
University of California, Los Angeles
Cornell University
University of Houston
University of Maryland
University of Michigan
Ohio State University
Rice University
Stanford University
Texas A&M University
University of Utah
Washington University
University of Wisconsin

CERN, European Laboratory for Particle Physics (Europe)
Deutsches Elektronen-Synchrotron DESY (Germany)
National Laboratory for High Energy Physics, KEK (Japan)

from which individuals were drawn to participate in the R&D and conceptual design and cost estimating work. In addition, the foundation of our work was considerably broadened by the appointment of a large number of expert panels and task forces to advise on matters of both policy and substance. Table 2 lists the institutions from which the required individuals were drawn.

We have been hard at work on our assignment since late 1984. The conceptual design and cost estimate that have emerged are captured in a five volume Conceptual Design Report, SSC-SR-2020, available from the DOE. Scientific, technological and cost aspects are treated therein. The design is shown schematically in Figure 1. Figure 2 is a plan view accompanied by a collage representing the various important technical and civil engineering components. This design, in which counter streaming beams of protons at an energy of 20 trillion electron volts each are caused to recirculate one above another by a superconducting magnetic confinement system and made to collide at six designated places, follows the now well established cascaded synchrotron collider concept. Several installations employing this general scheme, albeit on a smaller scale, have operated successfully. Thus the needed systems and processes are known from past experience and much expertise has been created upon which we could draw for the conceptual designs and cost estimates. We have done that. The conceptual designs of each technical system, sub-system and major component were made by recognized experts, as were cost estimates for those components. In important cases, detailed concept designs for components, such as magnets, were taken from working models to assure accuracy of detail. These permitted example manufacturing plans to be made by manufacturing companies

Table 2

Institutional Affiliations of Individuals who Participated in CDG Panels, Task Forces, and Workshops*

Argonne National Laboratory
University of British Columbia
Brookhaven National Laboratory
California Institute of Technology
University of California, Berkeley
University of California, Santa Barbara
University of California, Santa Cruz
Centre d'Etudes Nucleaires de Saclay
CERN European Laboratory for Particle Physics
University of Chicago
University of Colorado
Columbia University
Cornell University
Deutsches Elektronen Synchrotron
DOE, Chicago Operations
Fermi National Accelerator Laboratory
Harvard University
University of Heidelberg
Lawrence Berkeley Laboratory
Los Alamos National Laboratory
University of Michigan
National Laboratory for High Energy Physics (KEK)
Oak Ridge National Laboratory
University of Pennsylvania
Rutherford Appleton Laboratory
Stanford Linear Accelerator Center
Stanford University
State University of N.Y., Stony Brook
SSC Central Design Group
Swiss Federal Institute of Technology
Texas Accelerator Center
University of Tokyo
TRIUMF
Tsukuba University
University of Victoria, British Columbia
University of Wisconsin

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* There was also important participation by individuals from private industry and business.

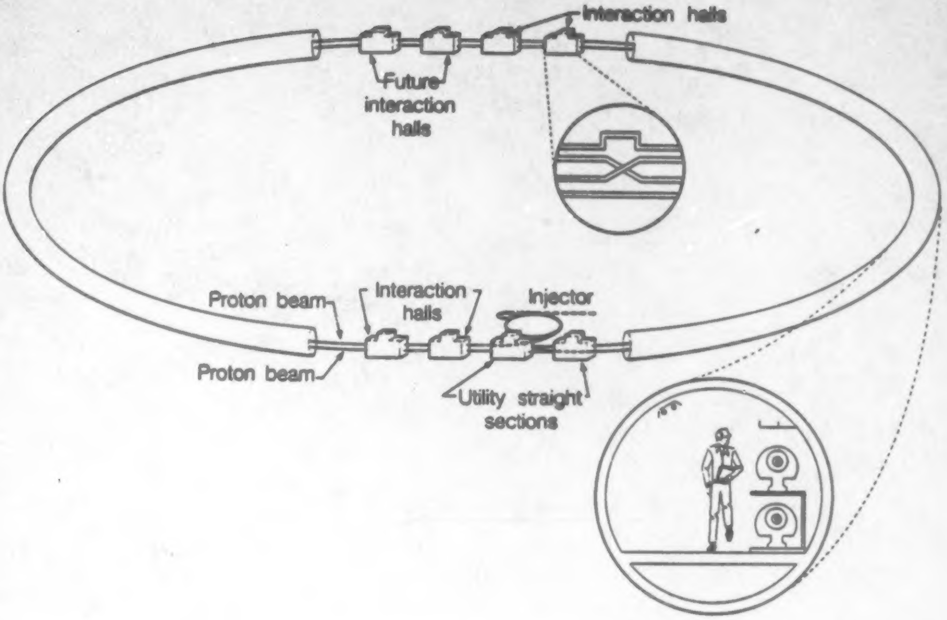
Interaction halls
Future interaction halls
Proton beam
Interaction halls
Injector
Proton beam
Utility straight sections

Fig. 1. Schematic layout of the SSC. The large ring represents the underground tunnel that houses the two collider rings and is 85 km in circumference. The clusters of facilities provide for injection and acceleration of the proton beams, and for physics experiments in the six interaction halls (four to be developed initially). The tunnel cross section (lower right) shows the magnet rings, one above the other. The enlargement inside the collider rings indicates how the oppositely moving beams of protons are made to collide. The distance between centers of interaction halls is 2.4 km.

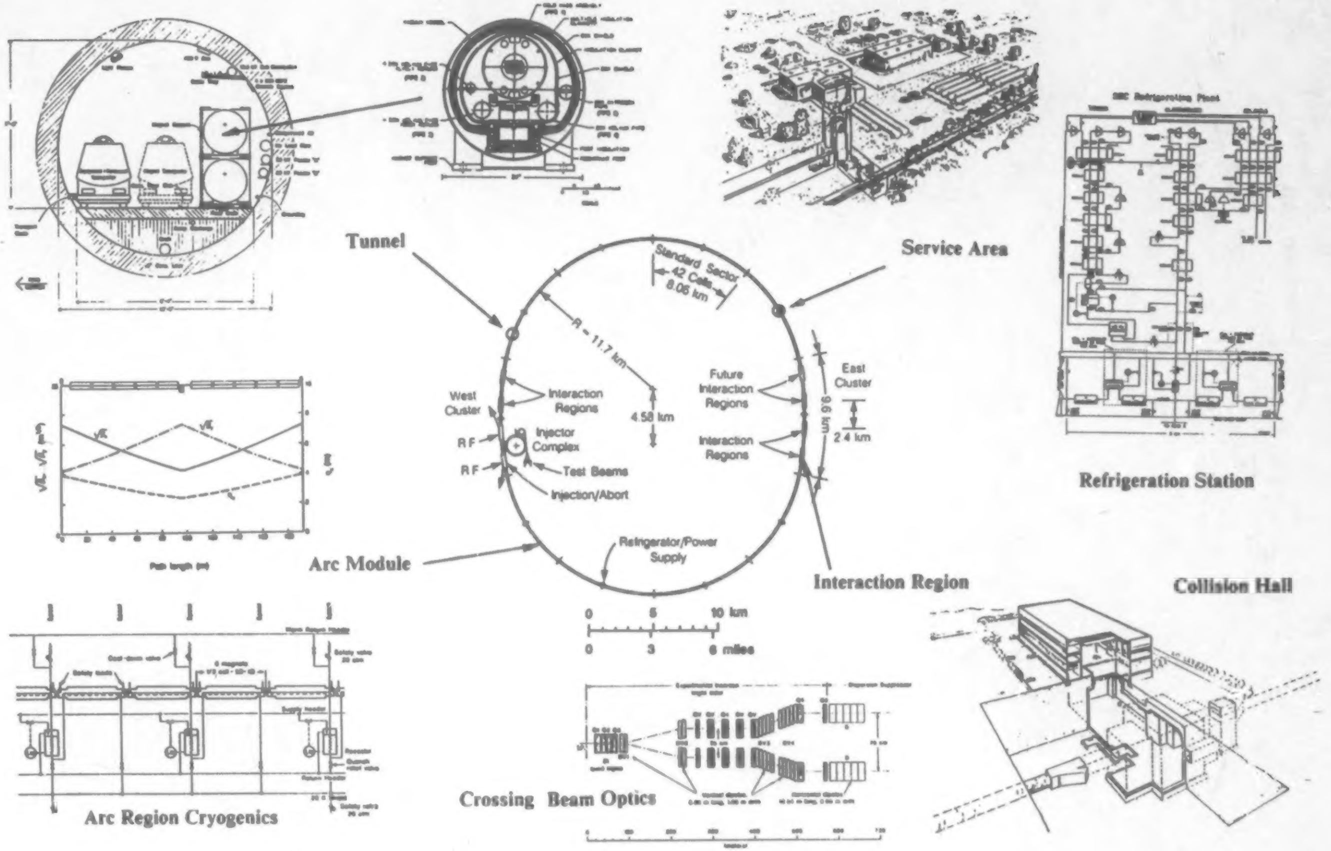
Tunnel
Service Area
Refrigeration Station
SSC Refrigerating Plant
Standard Sector
42 Cells
8.06 km
R = 11.7 km
4.58 km
West Cluster
East Cluster
9.6 km
2.4 km
Interaction Regions
Future Interaction Regions
Interaction Regions
Injector Complex
R F
R F
Test Beams
Injection/Abort
Refrigerator/Power Supply
Arc Module
Interaction Region
Collision Hall
0
5
10 km
0
3
6 miles
Path length (m)
Crossing Beam Optics
Arc Region Cryogenics

Fig. 2

for the purpose of detailed estimating of assembly as well as sub-component costs. These cost estimates were checked in detail by independent experts.

Most of the SSC will be placed underground and be invisible. A few small stations placed regularly around the arcs of the ring, as shown in Figure 2, break the surface. More buildings, giving above-ground evidence of the collision halls beneath, are to be seen at the interaction region clusters. The principal conventional systems of the SSC are the 53 mile underground tunnel and the collision halls. Accordingly, an extensive study of these systems was made as part of the conceptual design and cost estimate. In view of the unknown location of the eventual site a broad survey of possibilities was undertaken. Characterizations of the topological and geological distributions to be found in the United States were studied. It was found that by making three quite different conceptual designs for the civil works, examples covering most of the United States were in hand. In working out the examples the particular types of construction difficulties that inhere to the three different types of site were taken into consideration. Detailed simulation models for construction in these various circumstances were put together and tested on actual jobs that had been carried out to verify the accuracy of their predictions.

The conclusions were that construction costs for the three widely different types of site are quite similar. The cost differences are likely to be dominated by individual peculiarities at actual sites rather than their generic type. Thus we can be sure that there are many satisfactory sites across the United States; prevailing local details will have to be used to sort them out.

In the area of technical systems, over 6,000 individual items were considered in the cost estimate, including the needed engineering, supervision and contingency. These are reported in the 900 page Volume D of the Conceptual Design Report (CDR). Tables 3, 4 and 5 show a small example of the accounting system or Work Breakdown Structure used to develop the costs systematically. In the area of conventional systems, buildings, tunnels, utilities and the like, 3500 elements were considered. They are reported in some 300 pages of Volume C of the CDR.

The results of the cost estimate are presented in Table 6, and include all things necessary to construct the SSC. The total, exclusive of the costs for obtaining that site, is 3.210 billion in FY 1988 dollars. Figure 3 shows how the costs divide among the various categories.

Additional costs are involved in preparing for construction and operation for physics research. These are shown in Table 7 and encompass R&D in support of construction, pre-operating costs and the initial complement of detector and computer equipment. The sum of these items comes to 1.165 billion in FY 1988 dollars giving a grand total of $4.375 (FY1988). In addition the annual operating cost of the SSC facility has been estimated. Including operating and service personnel, materials and supplies, utilities, accelerator and detector equipment and anticipated accelerator and plant improvements the estimate comes to 270M ($FY1988) annually. The conceptual design and its accompanying cost and schedule estimates have been extensively reviewed by the DOE and the concerned technical community. They were found to be thorough and credible.

Work Breakdown Structure Levels

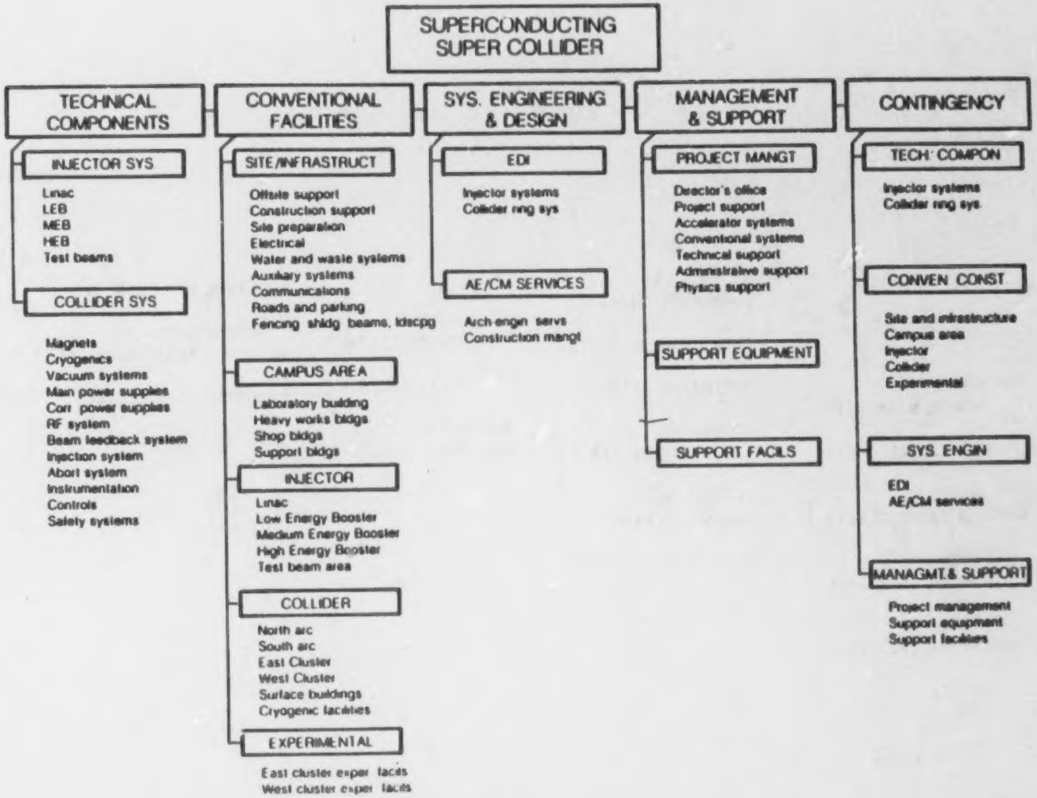
SUPERCONDUCTING SUPER COLLIDER
TECHNICAL COMPONENTS
INJECTOR SYS
Linac
LEB
MEB
HEB
Test beams
COLLIDER SYS
Magnets
Cryogenics
Vacuum systems
Main power supplies
Corr power supplies
RF system
Beam feedback system
Injection system
Abort system
Instrumentation
Controls
Safety systems
CONVENTIONAL FACILITIES
SITE/INFRASTRUCT
Offsite support
Construction support
Site preparation
Electrical
Water and waste systems
Auxiliary systems
Communications
Roads and parking
Fencing shldg beams, ldscpg
CAMPUS AREA
Laboratory building
Heavy works bldgs
Shop bldgs
Support bldgs
INJECTOR
Linac
Low Energy Booster
Medium Energy Booster
High Energy Booster
Test beam area
COLLIDER
North arc
South arc
East Cluster
West Cluster
Surface buildings
Cryogenic facilities
EXPERIMENTAL
East cluster exper facils
West cluster exper facils
SYS. ENGINEERING & DESIGN
EDI
Injector systems
Collider ring sys
AE/CM SERVICES
Arch-engn servs
Construction mangt
MANAGEMENT & SUPPORT
PROJECT MANGT
Director's office
Project support
Accelerator systems
Conventional systems
Technical support
Administrative support
Physics support
SUPPORT EQUIPMENT
SUPPORT FACILS
CONTINGENCY
TECH COMPON
Injector systems
Collider ring sys.
CONVEN CONST
Site and infrastructure
Campus area
Injector
Collider
Experimental
SYS ENGIN
EDI
AE/CM services
MANAGMT.& SUPPORT
Project management
Support equipment
Support facilities

Table 3

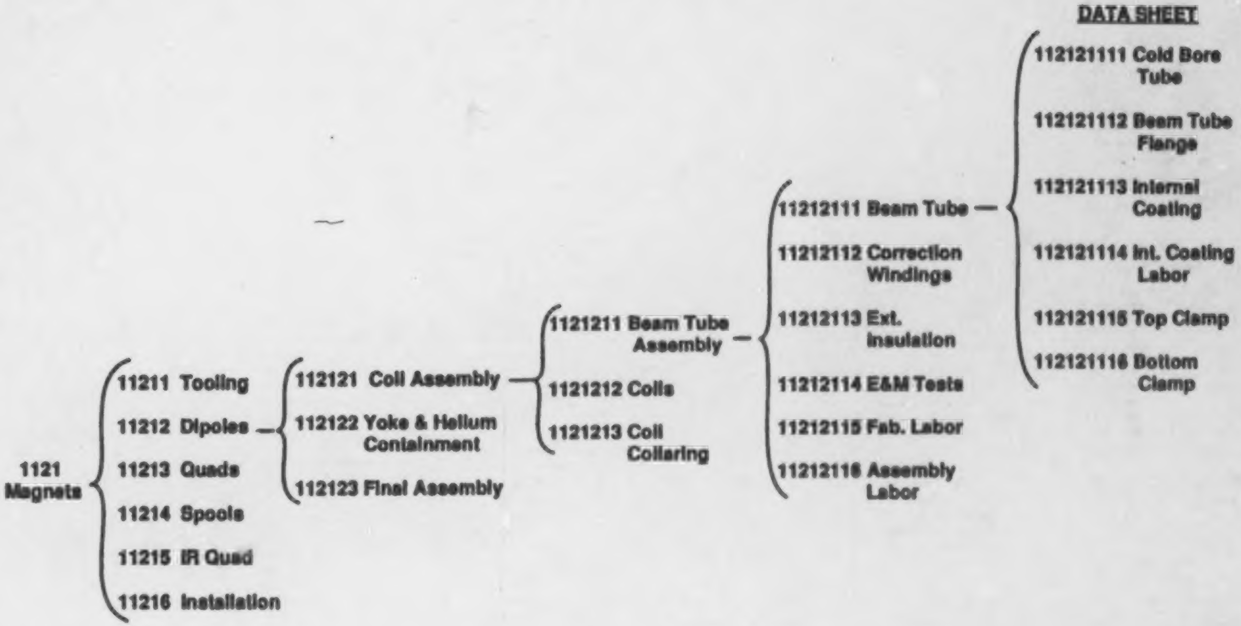
DATA SHEET
1121 Magnets
11211 Tooling
11212 Dipoles
11213 Quads
11214 Spools
11215 IR Quad
11216 Installation
112121 Coil Assembly
112122 Yoke & Helium Containment
112123 Final Assembly
1121211 Beam Tube Assembly
1121212 Coils
1121213 Coil Collaring
11212111 Beam Tube
11212112 Correction Windings
11212113 Ext. Insulation
11212114 E&M Tests
11212115 Fab. Labor
11212116 Assembly Labor
112121111 Cold Bore Tube
112121112 Beam Tube Flange
112121113 Internal Coating
112121114 Int. Coating Labor
112121115 Top Clamp
112121116 Bottom Clamp

Table 4

```
Page No.    29
04/01/87
                                        COST ESTIMATE DETAIL LISTING
                                        ==== ======== ====== =======

  1.1.2.1.2.1.1.1    DIPOLE, BEAM TUBE
  ===============    ======= ==== ====

  ITM ITM                           UNIT      NBR        UNIT COST     TOTAL  LBR      HRS/     TOTAL CRAFT CRAFT     TOTAL      ISC +
  NBR DESCRIPTION                   MEAS    UNITS        COST BASIS    ISC K$ UNTS     UNIT       HRS CODE   RATE  LABOR K$   LABOR K$
  --- -----------                   ----    -----        ---- -----    ------ ----     ----     ----- ----- -----  --------   --------


    1 COLD BORE TUBE                FT         60        7.60  VQ      0.456     1      1.0       1.0   T1  23.46     0.023      0.480
    2 BEAM TUBE FLANGE              EA          2       14.93  EU      0.030     2      0.2       0.5   T1  23.46     0.012      0.042
    3 INTERNAL COATING              EA          1      108.77  VQ      0.109     1      3.5       3.5   T1  23.46     0.082      0.191
    4 INTERNAL COATING                          0        0.00          0.000     1      0.2       0.2   T2  35.19     0.009      0.009
    5 BORE TUBE CLAMP,TOP           EA          1       30.93  VQ      0.031     1      1.0       1.0   S1  40.52     0.041      0.071
    6 BORE TUBE CLAMP, BOTTOM       EA          1       14.93  VQ      0.015     1      1.0       1.0   S1  40.52     0.041      0.055

 *** Total ***                                                         0.641                      7.2                 0.207      0.848

BRIEF NOTES / TECHNICAL DESCRIPTION OF THIS WORK PACKAGE:
---------------------------------------------------------
BEAM TUBE ASSEMBLY, PLATED INTERNALLY WITH .004 INCH OF COPPER, END FLANGES
FOR ATTACHMENT OF BEAM TUBE BELLOWS AND CLAMPS FOR ATTACHMENT OF TRIM COIL WIRES.


          ISC = INDUSTRY SUPPLIED COMPONENTS
          VQ  = VENDOR QUOTATION
          EU  = EXPERIENCE ESTIMATE (UNDOCUMENTED)
```

Table 5

Table 6

Project Cost Summary

WBS				FY 1988 M$
1.	**Superconducting Super Collider**			3,210
.1	**Technical Components**		1,518	
.1.1	Injector systems	201		
.1.2	Collider ring systems	1,317		
.2	**Conventional Facilities**		615	
.2.1	Site and infrastructure	91		
.2.2	Campus area	46		
.2.3	Injector facilities	42		
.2.4	Collider facilities	370		
.2.5	Experimental facilities	66		
.3	**Systems Engineering and Design**		307	
.3.1	EDI	209		
.3.2	AE/CM services	98		
.4	**Management and Support**		205	
.4.1	Project management	122		
.4.2	Support equipment	56		
.4.3	Support facilities	27		
.5	**Contingency**		565	

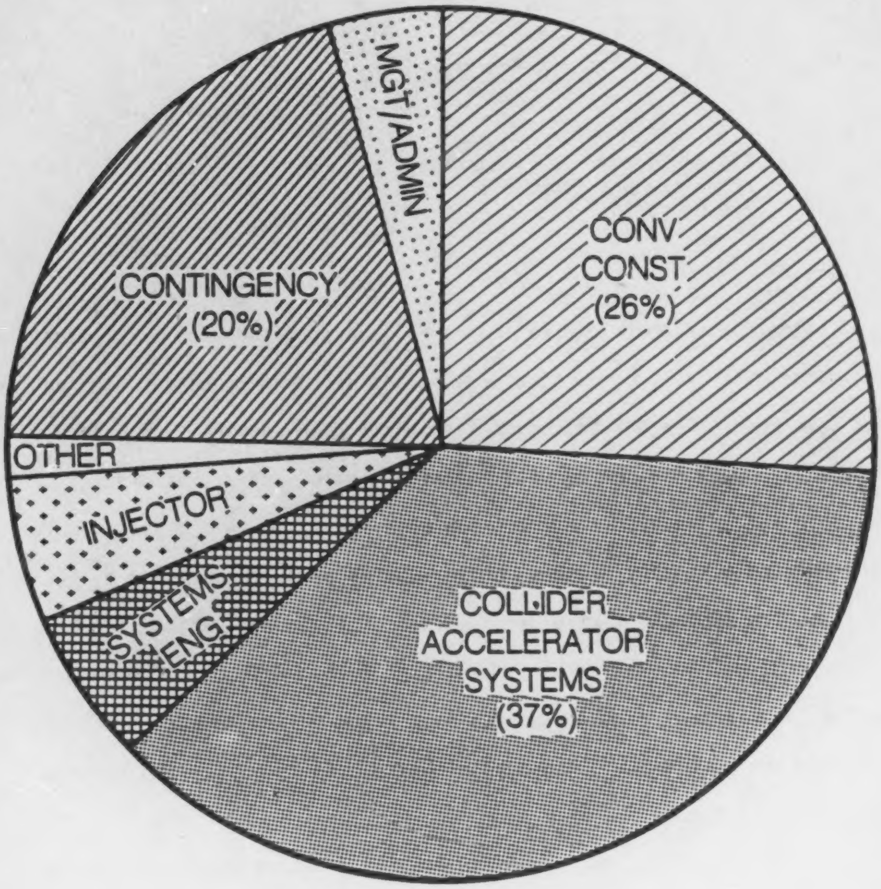
MGT/ADMIN
CONV CONST (26%)
CONTINGENCY (20%)
OTHER
INJECTOR
SYSTEMS ENG
COLLIDER ACCELERATOR SYSTEMS (37%)

Fig. 3

Table 7

Additional Costs

	FY 1988 (M$)
R&D in Support of Construction	274
Detectors and Computers	719
Pre-operating	172
Total	1,165

In the CDR a schedule duration of 6.5 years, based upon assumptions about the difficulty of construction at the site and precursor R&D funding, was developed. The DOE has suggested an 8.5 year construction schedule. Figure 4 shows a bar chart, with the time flow of activities meeting that schedule including DOE site selection activities. A very detailed model construction schedule, matched to the funding profile published by DOE on February 10, 1987 and taking detailed technical considerations into account, is in preparation. Naturally it will have to be revised to accommodate the particulars of the chosen site when available. Given the projected funding profile, the duration would probably not be affected, only the internal detail of activities.

As a result of the above related studies of end use need, technical accelerator questions and civil engineering considerations, broad general criteria for a suitable siting were produced. An early version was published as an SSC report in June of 1985. As the conceptual design was completed the revised particulars of the siting criteria were incorporated by the DOE into a comprehensive document called the Invitation for Site Proposals, written by the DOE and published on April 1, 1987.

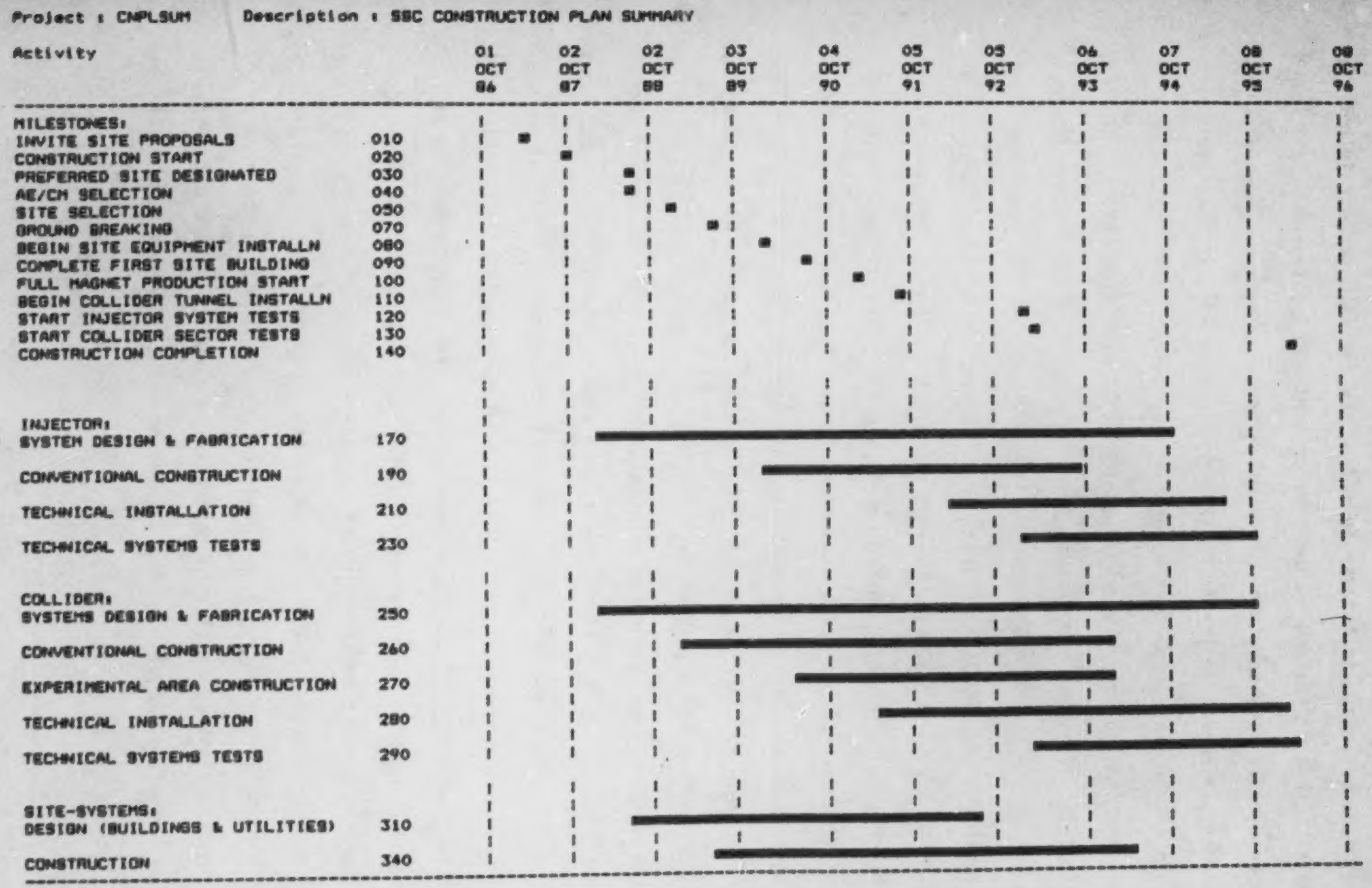
Project : CNPLSUM
Description : SSC CONSTRUCTION PLAN SUMMARY
Activity
01 OCT 86
02 OCT 87
02 OCT 88
03 OCT 89
04 OCT 90
05 OCT 91
05 OCT 92
06 OCT 93
07 OCT 94
08 OCT 95
08 OCT 96
MILESTONES:
INVITE SITE PROPOSALS 010
CONSTRUCTION START 020
PREFERRED SITE DESIGNATED 030
AE/CM SELECTION 040
SITE SELECTION 050
GROUND BREAKING 070
BEGIN SITE EQUIPMENT INSTALLN 080
COMPLETE FIRST SITE BUILDING 090
FULL MAGNET PRODUCTION START 100
BEGIN COLLIDER TUNNEL INSTALLN 110
START INJECTOR SYSTEM TESTS 120
START COLLIDER SECTOR TESTS 130
CONSTRUCTION COMPLETION 140
INJECTOR:
SYSTEM DESIGN & FABRICATION 170
CONVENTIONAL CONSTRUCTION 190
TECHNICAL INSTALLATION 210
TECHNICAL SYSTEMS TESTS 230
COLLIDER:
SYSTEMS DESIGN & FABRICATION 250
CONVENTIONAL CONSTRUCTION 260
EXPERIMENTAL AREA CONSTRUCTION 270
TECHNICAL INSTALLATION 280
TECHNICAL SYSTEMS TESTS 290
SITE-SYSTEMS:
DESIGN (BUILDINGS & UTILITIES) 310
CONSTRUCTION 340

Fig. 4

It is appropriate to observe here that the SSC is a project in one field of physics, but it resides in the broader context of research of all kinds and can benefit from developments in other areas. There has been tremendous excitement in recent months in the field of superconducting materials because of the discovery of rare earth copper oxides that remain superconducting to much higher temperatures than achieved before. The applications of higher temperature superconductors have been much discussed even before the latest developments. There certainly is a potential for revolutionary changes in many areas of technology. Large particle accelerators like the SSC may well also benefit, although they have very special needs. The history over the past twenty years of the development of the present multifilamentary, high-current-capacity strand, suitable for fabrication of accelerator magnets suggests the need for considerable R&D effort to get from the present exciting developments to thousands of miles of useful strand for a project like the SSC.

Yet we surely would wish to exploit advantages of new materials where appropriate. As is suggested by my second figure, the SSC is a very complicated instrument, with many, many interdependent technical components. The design is an optimization with respect to technical performance and cost of all these sub-systems. New developments in superconducting materials may impact somewhat the magnet design, but the consequences must be tracked through the whole project to assess the overall benefits. We are keenly aware of the possible potential of these new materials and have already strengthened our existing R&D in the area of improvement of superconductor to hasten the day when a reliable assessment of their potential benefit to the SSC can be made. The design and schedule of construction are flexible enough, we believe, to take advantage of rapid developments, if they occur. If, as I would guess, the realization of useful material for accelerator magnets takes a considerable number of years, the working SSC can benefit at some future time through a significant enhancement of its research power.

The technical feasibility and cost of the SSC have been established. As a result of the underlying R&D and conceptual design studies that have been accomplished, a strong foundation for beginning detailed engineering design and development and for expanding the already considerable industrial involvement has been laid. Thus, if approved, the SSC is ready to move forward.

In closing may I quote from a recent speech to the Materials Research Society by Dr. George Pake, then Group Vice President of the Xerox Corporation. In referring to the SSC he said:

> "Why do I favor the major investment -- this gargantuan particle physicists' 'boondoggle' or whatever worse name its critics may give it? How can a hard bitten industrialist, with all his pragmatic concerns, be an outspoken advocate of such a large and costly project that has so little direct economically tangible return? It is simple: For the near term and long range future vigor and benefit of the U.S. industrial and even military economy, the most pragmatic posture I can imagine is one that opts by overt choice for the most advanced knowledge and deepest understanding. For the United States instead to deliberately choose a 'me-too' or 'also-ran' research science status is to opt for ignorance rather than knowledge. Nothing is more pragmatic than the broadest and deepest knowledge base mankind can attain. And I am just chauvanistic enough to believe that the welfare and economic vigor of the United States is best assured if we lead the world in advancing knowledge and in developing that broad, deep knowledge base."

Senator **Ford**. Thank you, doctor.

Dr. Weinberg, I read your background and I kind of stood in awe of it, if you do not mind me saying that publicly. And then when I listened to you enthusiastically make your statement here today, I am more deeply impressed, if I can be.

So I am trying to hang on.

Dr. **Weinberg**. Thank you, sir.

Senator **Ford**. I am trying to hang on up here, to listen to you two gentlemen, and having to make the decision as to where we go. It is a burden that is pretty heavy for me to bear. But I hope through these hearings and listening to people like you, that hopefully we can make the right decision.

I have always been one that, if we have to build a few white elephants, we have got to try, we have got to keep going. And if we miss one, well, we set it aside and go on a little bit further.

It has been said that today's generation of physicists is producing a revolution of the understanding of nature that is comparable to the revolutions that occurred early on: Isaac Newton, Einstein's theory of relativity, and the twenties development of the quantum physics.

So would you comment with emphasis on the role now that SSC would play maybe in comparison to those, as you said, that you would leave Spain for the West Indies unless you found something more interesting. Well, they found America, and it has to be more interesting than any other place.

So can you comment and help me a little bit with that?

Dr. **Weinberg**. Yes. Thank you, Senator.

Well, we like to talk about revolutions in science, although we're really basically peaceable folk. And as you correctly said, we look back at the advent of Newtonian physics, relativity at the beginning of the century, and then quantum mechanics in the 1920's as among the greatest revolutions that have ever occurred.

We know there are problems with our understanding based on the information we have gleaned from those revolutions. For example, it's very hard to combine what we know about nature that we've learned from the revolution called quantum mechanics, which teaches us about the way things work in the very small, with what we know about gravity, which Isaac Newton first taught us about in a serious scientific way, and then Albert Einstein provided us with the modern theory of.

Gravity doesn't seem to fit into a quantum framework. One of the questions, one of the spots on the wall on which us theorists have been beating our heads, is the question of how to combine gravity with the rest of elementary particle physics.

There are fascinating ideas which are current right now on how to do that, ideas that have implications for the rest of elementary particle physics, ideas that have a chance of explaining all those mysteries which were left mysterious by the standard model—mysteries like why the electron weighs as much as it does and why the quarks are what they are.

But we haven't been able to do it by pure thought. This is what I meant in my statement when I said we're stuck. We've got this fascinating problem of combining gravity with quantum mechanics.

We've got these nagging open questions about why the elementary particles are the way they are.

There's some kind of intellectual synthesis waiting that will make sense out of it all, and the theorists like myself have not been able to do it without the impetus provided by the new accelerators that we'll need.

And I believe that we will need the SSC to carry us to this next step. If I can add one more word about this, it's a word I used earlier, esoteric. These things are awfully esoteric. I don't think there are an awful lot of citizens, either in Texas or Kentucky, who care very much about combining quantum mechanics with gravity.

However, it isn't that long before the answers that we get actually do become part of the ordinary equipment of citizens in general. I mean, if you stop any passer-by on the street and say, and ask him: Sir, do you know that the earth goes around the sun, or do you know that ordinary matter is composed of atoms, or you can even ask him, do you know that it's possible to convert mass into energy, he or she is likely to say: Well, we all know that; that's obvious; why are you bothering to tell me that?

These things were very esoteric when they were first discovered. Now they're part of our everyday sensibility. They're part of our civilization. I think it won't be that long before these problems that the elementary particle physicists are working on are things, are matters that are discussed in elementary school.

Senator FORD. Let me ask Dr. Tigner a question since he seems to be the builder here. Should we be building the SSC because of the economic spinoff from high energy physics or should we build it because of the spinoff in understanding of basic science from the SSC?

Dr. TIGNER. Thank you. I think there is no doubt in my mind whatsoever that the primary goal of this activity is as Dr. Weinberger has just expressed to us. The scientific pushing forward with scientific knowledge is by far and away the most important contribution that this activity makes to our culture and eventually to our economy.

Senator FORD. Let me ask you, then, the cost of operation. Operating cost is usually 10 to 20 percent of construction cost, when we get into what we understand here in dollars and cents. Your estimate is less than 6 percent or so.

Are you sure the operating cost is not going to be higher?

Dr. TIGNER. Yes, I think that is an interesting question.

Senator FORD. I meant it to be, because that is one thing that has to be answered. We have a rule of thumb, you know, if it is so much, so much. And we need to be sure, because this is so much smaller than the other.

Dr. TIGNER. Yes. Well, I think that this—first of all, when one applies rules of thumb, one has to be careful that the systems that the rules of thumb came from are comparable to the systems one is discussing. And I think that, first of all, the scale of the SSC is such that one might expect—and we went into this question in great detail ourselves, and we came up with our answer. We said, hey, can we believe this?

And we went through the similar kinds of arguments that you're talking about in more detail. And of course, we examined the history of other facilities of a similar kind. And I think that one can see that this characteristic of these very large instruments is such that it's gotten beyond that simple rule of thumb.

And the operating cost really has much more to do with the technology which sets the consumption for power and other supplies and the number of people who are being served by the facility, rather than the capital cost of the facility itself.

And I think that if one goes and compares—one of the important checks that we have on our correctness of our estimate was to go and check all the other facilities of a similar, that do a similar kind of science, both in this country and abroad, and see whether we could verify our calculations from the ground up on their system and get the same answer.

And the answer is that we did. So we were happy that our method of deriving the operating costs checked out on existing facilities that do the same kind of science, and address roughly the same kinds of numbers of people, and consume roughly the same kinds of numbers of consumable supplies.

You should appreciate that the technology which makes the SSC possible makes an enormous dent in the amount of electrical power that's required to operate the facility. For example, the operating power for the SSC as now designed will be somewhat less than required to operate other high energy physics facilities in this country at this time. You can see there's an enormous saving by use of this technology.

Senator **Ford**. What is a comparable facility to SSC? I did not think we had anything out there that was comparable?

Dr. **Tigner**. Well, the Tevatron in Fermi Lab. For example, you could ask what is the operating cost there compared to the capital cost of that facility. And today the operating cost of that facility is about $140 million per annum.

It serves a number of people which we guess is about half per annum that will be served by the SSC. And its capital cost in today's dollars is very close to a billion dollars.

Senator **Ford**. As Dr. Weinberg so eloquently said, this is reaching out for another answer that may bring us a little closer to what we have been researching for, hopefully in his lifetime and mine. Mine is a little bit shorter than his because I am so much older.

Are you really confident that this $4.4 billion estimate will hold, that your $4.4 billion estimate will hold? Others say it will go as high as $10 billion in order to give Dr. Weinberg the opportunity to reach out and find the other answer.

We are talking about $4 billion or $10 billion, and the way this place operates we can come a lot closer to $4 billion than we can to $10 billion, even though we talk in trillions up here. But can you hold to that line of $4 billion, $4.4 billion?

Dr. **Tigner**. Yes, sir, that is a very serious question, and we have taken it in a very serious vein. As you have observed—I left it on the floor there, but all the way up on the bottom of that chair there is the conceptual design report, and cost estimate was taken very seriously.

Now, why can I be confident that that cost estimate has reality about it? Well, in comparing a project like this to others in which—other large technology projects where one has had unpleasant surprises about the ultimate cost, I think by contrast one can see that all of the processes, all of the components for the SSC follow well-known technologies.

There are no new, entirely new processes, or new subsystems that have to be invented from scratch, so that we don't know what we might be up against. We have experience, very firmly based experience upon which we can make these judgments.

The individual components, although when put together come to a much larger scale than we contemplated before, in and of themselves are very comparable to things that have been built in the past. And as I said, we went to great length to break these things down into their pieces and to consult with a large number of experts who had designed and who had built very similar things in the past.

And then we have those results checked by independent people. So I am confident that we have done everything we possibly can to do a responsible job, and with all the checks and balances that we were able to install in this elaborate cost estimate, we have in fact obtained an accurate answer. We took it very seriously for the reason that you mentioned.

Senator FORD. Doctor, we take everything seriously around here, but then there is a certain item called overruns. And when we get into overruns, sometimes they cost as much as the original estimate.

And we have just a small simple item around here: When the Architect of the Capitol gives us an estimate, you know it is going to cost about twice that. So we have to have a supplemental.

So I want to be sure that when we get into this field that—I may not be able to comprehend everything you are talking about out there, but I am beginning to understand what you are trying to do, what answers you are looking for. And I have to have the ability to find a way to give you that opportunity.

And as you heard earlier from DOE, they have four projects that they would prefer to move forward with before we get into the SSC. And the White House has recommended that we go forward with this, talking about $5 billion. And yet, they want us to cut the budget. And this is an addition to the budget, and where do you find the money?

So the problem is to be able to give you the opportunity to move forward with it.

I appreciate you all coming here today. It has meant a great deal to me personally. I hope it has meant a little bit to you. We are going to move forward with this and do the best job we can.

We have testimony over in the House still going on, and our final witness is still over there and will not be here. So we will take Daniel Kleppner's testimony in writing.

[The prepared statement of Dr. Kleppner follows:]

Testimony presented to the

Subcommittee on Energy Research and Development
Committee on Energy and Natural Resources
United States Senate

April 7, 1987

by
Daniel Kleppner
Department of Physics
Massachusetts Institute of Technology

Mr Chairman and member of the Committee:

Thank you for the opportunity to discuss the question of how the Superconducting Super Collider is viewed by the non-high energy physics community, and the SSC's likely role in the advancement of science. The proposed SSC represents a dramatic commitment to basic science. An enterprise of such a magnitude cannot help but impact a much greater community than its immediate users; indeed, such an investment could hardly be justified if its purpose were solely to satisfy the intellectual curiosity of the world's population of high energy physicists. The argument for the SSC fundamentally rests on the generative role of particle physics in the advance of science. This argument has been posed eloquently by the SSC's proponents and I will not attempt to repeat it here. Rather, my goal today is to address the more general role of the SSC in science, and how it is viewed by the large fraction of physicists who are not actively engaged in particle physics.

I emphasize that these comments reflect my personal opinions as a university-based non-high energy physicist: I am not speaking today on behalf of any organization or group. However, my activities during the past few years with respect to planning for the future of science in the United States, particularly my experience on the Physics Survey Committee of the National Research Council, have put me in contact with many members of the physics community.

The particle physicists have endorsed the SSC with enthusiasm, but, as is to be expected, not every physicist shares their vision. It would be presumptuous to attempt to represent every point of view of the physics community, but I will attempt to sketch the major arguments.

A DEEPLY OPPOSED MINORITY

Some scientists are fundamentally opposed to the SSC on the grounds that there is not enough scientific interest to justify an expenditure of this magnitude at a time when the U.S. faces an unprecedented challenge from abroad. They argue that the payoff to National needs in terms of technological advance or increased competitiveness is too remote to warrant such a project. The most extreme view of this camp is that particle physics represents a diversion of needed scientific talent from areas that would be more productive for the Nation.

Although such severe misgivings about the SSC are expressed by some highly respected members of the physics community, I think that it is fair to say that this is a minority viewpoint. The majority of the community is supportive to varying degrees, but the support is prefaced by an emphatic *If*...

AN UNEASY MAJORITY

Particle physics is widely recognized as one of the most intellectually challenging and exciting areas of physics. The recent advances in our understanding of the basic structure of matter cannot fail to stir the spirit of physicists everywhere, and one would have to be jaded indeed not to be excited by the prospect of a unified theory of the fundamental forces of nature.

Supporters have emphasized that the SSC should be funded with new money, However, there is concern that a project that will go through a change of administration and several Congressional elections will lose its "new" label in the yearly budget deliberations; Congress will simply note that support for science is "up". For this reason it is feared that the SSC could have a negative impact on other areas of science. If it were not for this concern, the overwhelming majority of the physics community would undoubtedly give the SSC their blessing.

ONE PHYSICIST'S VIEW

Let me present the arguments as I see them.

For the United States to turn its back on a major area of physics would be a signal that science is losing its importance, and that the Nation is giving up its aspirations to explore the frontiers of knowledge. John Donne's admonition "No man is an island, entire of itself" holds true for science: if we sacrifice one vital field of physics, every field of physics will be affected, and ultimately all of science will be diminished.

The U.S. particle physics community, after long debate, has judged that the future of particle physics lies in the SSC. Without having participated in these debates, it is difficult for a non-high energy physicists to judge all the issues involved. Nevertheless, the particle physics community, which includes some of our most distinguished scientists, has voted for the SSC with impressive unanimity.

Personally, I view the SSC as one of the most exciting projects that the Nation could undertake. As a monument to our National aspirations, as a signal to youth of the value of science, as a rallying point for international collaboration, the SSC provides the Nation with a unique opportunity. However, it is essential to realize that construction of the SSC will not by itself assure that the U.S. will maintain eminence in science, nor even that the U.S. will maintain the minimum level of physics required to meet its needs. Today, in the United States, the physical sciences face serious problems. Because of this, I believe that the SSC must be viewed in the largest possible context of the Nation's scientific future.

The United States has enjoyed scientific supremacy for so long that it seems almost inconceivable that we could slip from the first ranks, but recent studies of physics in the U.S. raise alarms. Education, particularly at the primary and secondary level, is a fundamental concern. More relevant to the discussion of the SSC, however, is the future of physics research as a whole. High energy physics is a visible and important part of the picture, but only one of many parts. Other major fields of physics include astrophysics, nuclear physics, plasma physics, condensed matter physics, and atomic, molecular and optical physics. In addition, interdisciplinary areas—biophysics, geophysics, surface science and medical physics, to name a few—are a vital part of physics today. All of these fields of physics are joined by conceptual and technical links. They reinforce each other, stimulate all of the other sciences, and constitute a single scientific enterprise. It is the health of this total enterprise that is essential to the Nation.

PHYSICS IN SMALL GROUPS

Only a small fraction of physics is organized around accelerators; most physical research is carried out by small research groups in universities, national laboratories and industrial laboratories. This style of research, which can be loosely called "small" physics, is so varied that it defies summarization: the goals range from the most fundamental problems on the organization of matter to the development of new materials and devices for technology.

The recent discovery of high temperature superconductors testifies to the scientific vitality of "small" physics, and the enormous economic benefit that can follow. Last spring, K.A. Müeller and J.G. Bedhortz, working at I.B.M., Zurich, in a literally two-man effort, discovered superconductivity in a new class of materials, ceramic-like oxides of copper. Their discovery has triggered a scientific explosion. In December, C.W. Chu and his group at the University of Houston reported that the transition temperature is far higher than in any other material. It has now been verified that the material can be superconducting above liquid nitrogen temperature, and there is hope that room temperature superconductivity can be achieved. The outpouring of ideas, the scientific excitement and the level of new activity in this area, are unique in the history of physics. The research has advanced so rapidly that a session on high temperature superconductivity at the meeting of the American Physical Society in New York last month that started at 8 P.M. continued without interruption to 3 A.M., and conversations were still going on at 6 A.M.!

The possibility of making superconducting devices without the need for liquid helium refrigeration could open the way to new technologies for transportation, energy and defense: levitated trains, more efficient magnets for plasma fusion, and electric motors and generators far more powerful and efficient than any yet produced. Magnetic resonance body imaging—the miraculous technique for taking "photographs" within the human body— stands to be vastly improved. Computers are likely to be among the first beneficiaries of the new technology: the high speed and low energy dissipation of superconducting circuits may provide revolutionary new types of computer elements. It is too early to know which if any of these potentials will be realized, but hope is running high.

The discovery of high temperature superconductivity testifies to the value of basic research by small groups of scientists who have the freedom and resources to pursue their scientific goals. This style of research is responsible for many discoveries in basic physics, as the frequency of Nobel Prizes attests, but its value stems from more than the advancement of pure science. "Small" physics encompass areas such as materials science, surface science, optics and polymers. These are the areas that most often spawn new technologies, that contribute directly to the National programs in energy, the environment, and defense. These are the areas that are closest to the needs of industry. Furthermore, these are the areas that train most of this Nation's professional physicists. Over 70% of our Ph.D. students carry out their research in small research groups in our universities. Should this style of physics wither, we will lose a driving force for new technology and most of our capability for training professional scientists.

Considering the contributions of "small" research to the Nation, it is disturbing that recent studies of physics in the United States cite deep-rooted problems, and express serious concerns for the future. These include the physics survey, *PHYSICS INTO THE 1990's*, published last year by the National Research Council, and *A RENEWED PARTNERSHIP*, the White House Science Council report on the Health of U.S. Colleges and Universities, also published last year. The chemistry survey, *OPPORTUNITIES IN CHEMISTRY*, published by the NRC in 1985, reached essentially the same conclusions for chemistry.

CONCERNS FOR UNIVERSITY-BASED RESEARCH

In the United States today, university-based research in physics is in a deteriorating situation. Equipment is obsolete, facilities are outmoded. Shops and support services, if they exist at all, are inadequate; operating funds are stretched thin. University research groups are being cut off from the research frontiers.

An exciting recent development in materials science is the creation of solids on a layer-by-layer basis in which the composition is controlled at the atomic level. The fabrication of these so-called deliberately ordered materials opens the way to new studies of the properties of solids, and the creation of new materials with unique electrical, thermal and mechanical properties. Industrial interest is intense. However, few universities can participate in the research: the million dollar price tag for the apparatus is too high. To cite another example of how the universities are being cut off from research frontiers, it is now possible to create laser light pulses so short that one can effectively take snapshots of molecules as they react, making it possible to witness how individual electrons move and rearrange themselves. The impact on molecular physics is enormous, and the technique is being applied to other disciplines such as surface physics and semiconductor physics. However, the laser systems cost several hundred thousand dollars, too much for most research budgets.

A typical university-based research group in experimental physics might consist of a professor, four or five graduate students, and a post doctoral worker. Such a group typically requires three to four hundred thousand dollars a year to operate. The average research grant is less than half of that.

The reports cited above go into considerable detail about the scientific opportunities and the problems facing university-based research. Rather than to repeat that material, let me present some fresh and disturbing evidence of a system headed for deep trouble,

In the United States, university research positions in physics have long been regarded as among the most prestigious and desirable. However, after a decade of rapid expansion in the 1960's, the universities severely cut back on new appointments. For the past fifteen years the rate of appointments has been a relative trickle. In fact, appointments have been so scarce for so long that it is generally believed that large numbers of qualified applicants are available for every university research position. This was true once, but it is not true today: a recent survey carried out by the American Physical Society reveals that in some areas of physics the universities are experiencing great difficulty in filling positions. The report has not yet been published, but a summary of the survey is attached.

The areas that cannot find candidates are experimental condensed matter physics, and atomic, molecular and optical physics. These two fields train approximately half of our Ph.D. students, and they are among the fields of physics that have the most immediate impact on societal and industrial applications. If the faculty positions remain unfilled, one can expect to see an impact on Ph.D. production in the coming decade.

In 1970 the median age of a physics faculty member was thirty nine years; today it is fifty years. If we are experiencing a shortage in faculty hiring today, what should we expect tomorrow when the retirement rate of these aging faculties starts to zoom?

A related consideration is the dwindling supply of U.S. graduate students. Although total Ph.D. production is relatively constant, this is only because of a steadily increasing component of foreign students. Today, over forty percent of our physics graduate students, and more than half of our postdoctoral researchers, come from abroad. We could not maintain our technological enterprise without this infusion of foreign talent. That we can attract and welcome good minds from all over the world testifies to the great strength of our system, but our rapidly growing dependence on foreign talent is cause for unease. This is a particular concern with respect to defense-related research, for which foreign-born scientists are generally ineligible.

This is the time when we should be making every effort to attract youth into science, and to attract our best young scientists to university careers. However, according to written comments on the A.P.S. survey questionnaires, the universities can no longer compete with industry for the best talent in condensed matter physics and atomic, molecular and optical physics. Higher industrial salaries are one factor, but industry has always paid more than academia. The most important factor appears to be the discouraging climate for university-based research.

It is difficult to think of a more ominous sign for science than this growing flight of our best young scientists from university research positions.

THE DOE, THE SSC, AND RESEARCH IN THE UNIVERSITIES

The President has called for a renewed commitment to science and technology in the United States to help counter the growing threat to our economic competitiveness. He has called for a broad initiative in basic science and technology for all of the research agencies.

The National Science Foundation is participating in the Presidential initiative by a five year program that is centered on the creation of science and technology centers in universities across the Nation. However, the NSF sponsors only about half of this Nation's university-based research in the physical sciences. The universities are dependent primarily upon DOE, DOD and NASA for the other half of the support.

The concerns of the small science community about the impact of the SSC are specially pertinent to its sponsoring agency, the DOE. This agency not only provides the principle support for particle physics in the U.S., it is a major supporter of small science in the universities. In particular, to support its energy mission, the DOE sponsors research in materials science, chemical science and a number of related areas through its program in Basic Energy Science. Next to the National Science Foundation, Basic Energy Science is the largest single source of support for university-based research in the physical sciences.

At the request of the Secretary of Energy, the Energy Research Advisory Board has reviewed the recent NRC survey reports in physics and chemistry. The first recommendation of the Physics Review Panel is

> "A major concern expressed by the NRC report, and strongly shared by the ERAB Panel, is the unsatisfactory condition of basic physics in the universities. The Panel recommends that DOE provide new funds for research and instrumentation to a number of small academic research groups performing research of high quality."

The thrust of the Chemistry Review Panel, in its concern for research in the universities and its emphasis on increased support for a broad program of basic science, is similar.

These Energy Research Advisory Board reports on physics and chemistry underline the critical need for strengthening university-based research in the physical sciences. They provide, in my opinion, an overwhelming argument for the DOE to participate in the Presidential initiative.

The Nation needs the SSC to maintain its leadership in particle physics. However, reassurances that the SSC will not adversely effect other scientific budgets miss what I believe is the crucial point: if our universities decline our scientific future will be dim. If our universities are not to decline, university-based research must receive new support. The Department of Energy has formulated a long range plan for the construction and operation of the SSC. Now is the time for the DOE to formulate a long range plan to help reverse the decline in "small" science through a major new commitment to its program in Basic Energy Sciences.

Attachment A

SURVEY ON THE ADEQUACY OF FACULTY CANDIDATES IN PHYSICS

The Panel on Public Affairs of the American Physical Society recently undertook a study of the physics workforce through an *ad hoc* committee co-chaired by Dr. Bernard G. Silbernagel of Exxon Research and Engineering Co. and Dr. Peter A. Wolff of the Massachusetts Institute of Technology. The committee's activities included a survey on the adequacy of faculty candidates in physics. The following is directly quoted from the report. (The attachments to the report are omitted.)

"The Survey of Department Chairs in Ph.D.-granting institutions was sent to the 197 such institutions in the U.S. and Canada. A copy of the questionnaire is attached for reference. Responses were received from 127 of the 172 U.S. physics departments surveyed. Such a response rate — 76% — is practically unprecedented for a study of this type and reveals widespread concern for the issues raised."

"A detailed description of the number of faculty openings and their distribution within various sized departments is summarized in the Appendix. We will focus the present discussion on the most illuminating question, the supply of candidates in the various physics subfields. The respondees were asked to describe the availability of qualified candidates for existing openings on a scale of 1 (abundant) to 5 (inadequate). The results are shown in Figure 1."

"In elementary particle theory there is an abundant supply of qualified candidates. In condensed matter theory, nuclear physics, experimental particle physics, and in plasma physics to a lesser extent, there appears to be a reasonable balance between the supply of candidates and the number of openings. By contrast, in condensed matter experimental physics it is evident that the departments are having difficulty in filling positions. Only 23% of the departments report an adequate candidate supply, while 43% rated the supply in category 4 and 28% termed the supply inadequate. The situation in Atomic, Molecular and Optical physics is only slightly better."

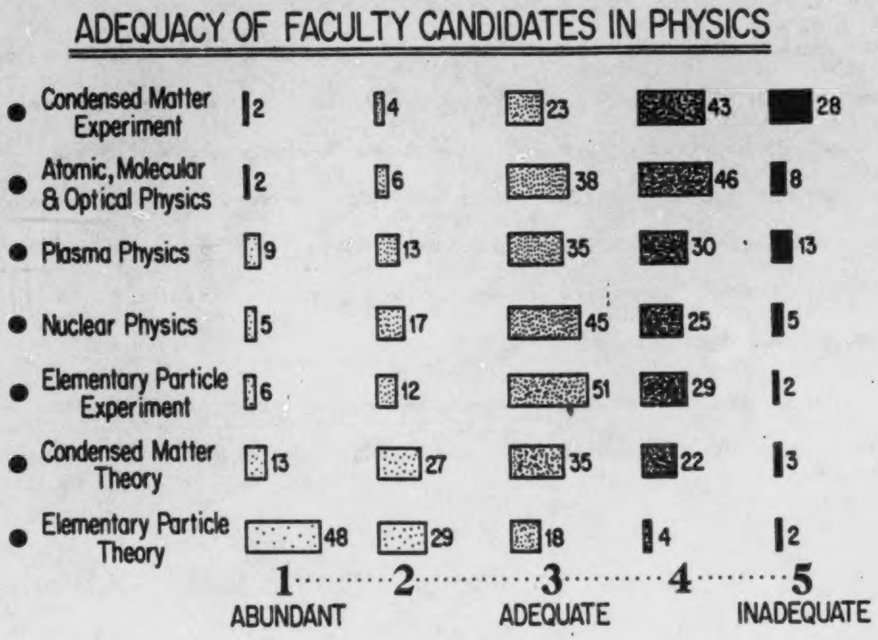

Figure 1.

"The real significance of these findings can best be appreciated in light of the anecdotal comments which accompanied them. When a respondee indicated that the supply was inadequate, the department had been trying to fill the position for an appreciable period of time."

"The lack of experimental condensed matter, as well as atomic, molecular and optical candidates cannot be ascribed to as fall-off in the Ph.D. production in these areas, for the statistics of the Brinkman

Report reveal that the production of Ph.D.'s in these areas has remained relatively strong. The shortage appears to be due primarily to the fact that there are excellent opportunities in these fields and that the universities are losing their ability to compete for the best candidates. We believe that this documented shortage of candidates is extremely significant because it is contrary to the widespread perception that university positions are highly desirable. The shortage cannot be dismissed merely on the grounds of salary competition from industry, for that has always existed. As mentioned above, it is also not due to a decrease in Ph.D. production. We believe that the universities are losing their attractiveness to young scientists. This is most clearly evident in areas where physicists have the best alternative research career opportunities and are pursing them. However, the Brinkman Report and other studies, such as the Packard-Bromley Report indicate that most areas are affected by the same problems: inadequate grant funding, outmoded equipment and deteriorating research infrastructure, low salaries, and the pressures associated with seeking tenure."

"Industry has particular reasons to be alarmed about the faculty shortage in condensed matter experimental physics. If these positions remain unfilled, it will not take long for the Ph.D. production in the area to decline."

Profile

Daniel Kleppner

Daniel Kleppner joined the Physics Department of the Massachusetts Institute of Technology in 1966 where he is now the Lester Wolfe Professor of Physics and Associate Director of the Research Laboratory of Electronics. Dr. Kleppner received the B.A. degree from Williams College in 1953 and Cambridge University in 1955, and the Ph.D. from Harvard University in 1960. He is a member of the National Academy of Sciences and a Fellow of the American Academy of Arts and Sciences, the American Physical Society and the American Association for the Advancement of Science. He has served as Chairperson of the Division of Atomic, Molecular and Optical Physics of the American Physical Society, and is currently a councillor of that Society. From 1983-85 he was a member of the Physics Survey Committee of the National Research Council. Dr. Kleppner's research interests are in experimental atomic physics, high precision measurements and quantum optics.

Research in small groups

Small-scale research plays a vital role in advancing physics, in training physicists and in generating new technologies, but we will need a new perspective to judge its needs alongside those for large facilities.

Daniel Kleppner

In Munich's Deutsches Museum one can come upon an Italian Rennaissance study, meticulously recreated and handsomely furnished save for one intrusion: A plank, one end propped on a trestle, the other resting on the floor. With such a crude apparatus and using his pulse to clock how long a brass ball takes to roll various distances, Galileo first explored the nature of uniformly accelerated motion. With the aid of these measurements he discovered the basics of dynamics; in so doing he also established a tradition for experimental research that has animated physics ever since.

Galileo created the style of a scientist who devises his own experiment, builds his own apparatus, takes his own data and draws his own conclusions. This is the style of Newton and Cavendish, of Fraunhofer, Faraday and Michelson, of Helmholtz, Roentgen, Rutherford and Millikan.

With the invention of accelerators, plasma machines and other large-scale facilities for research, a different style emerged—"big" physics—in which scientists conduct research in large groups and use facilities that are constructed and operated by others. Such research has generated some of the most exciting advances in physics during the past decades. However, the tradition of physics on a small scale—"small" physics—is also very much alive. Today's small-scale research is generally pursued in small groups rather than by individuals, and even the conceptually simplest experiments are likely to require complex and costly equipment. Nevertheless, this tradition has its share of the triumphs in physics.

This article attempts to describe the scope of small-scale research in the advance of physics and to show its role in training physicicts and generating new technologies. It will discuss the increasing problems that face small research groups—particularly in the universities—and that give cause for concern about the future of this style of research in the United States. It will then propose a fresh perspective that can provide a useful starting point for addressing these problems.

Because of its diffuse nature and varied goals, one could easily overlook physics on the small scale in viewing the enterprise of physics. But, as we shall see, one would overlook much of the best and most valuable of contemporary science, for this research plays a major role in the advances of physics. If the Nobel prize can be used as a guide to what is important and enduring in physics, it is in the areas of small-scale physics that much of the scientific action takes place. Furthermore, these are the areas where physics most frequently interacts with the other sciences and where it has the most direct impact on our national programs and on industry. Thus, viewed collectively, the independent groups assume a clear identity. These groups produce a large fraction of our best science, train over 70 percent of our graduate students and generate much of our new technology.

Big and small physics

The distinction between big and small physics stems not from their separate scientific goals but from the experimental style required for effective research. The frontier of particle physics—the archetype of big physics—lies at the highest possible energy that can be attained: Nearly all the experimental research must be carried out by large groups using giant accelerators. The frontiers of condensed-matter physics—the major field of small physics—lie in the study of materials by various types of spectroscopy and other techniques requiring small instruments: Such research is most effectively carried out in small groups. Atomic, molecular and optical physics also thrive in the small-group style, as do portions of nuclear physics, astrophysics and plasma physics. Fluid physics, acoustics and all of the "interface" areas, such as biophysics and medical physics, also belong to small physics.

Small-scale research sometimes requires large user facilities. Synchrotron light sources, for instance, are generally used simultaneously by many small groups. Each group sets its own research goals and supplies its own equipment; usually this research constitutes only one portion of the group's program.

One telling indicator of the difference between small and big physics is the number of authors contributing to a scientific paper. The average number of authors for a *Physical Review Letter* in condensed-matter physics is less than three; in particle physics it is more than 40.

In the United States, small-scale research in physics is organized around approximately 1800 independent groups, some as small as a single faculty member with one graduate student, or an experimenter with a few colleagues in a government or industrial laboratory. Many of these groups are relatively free to shift their goals as

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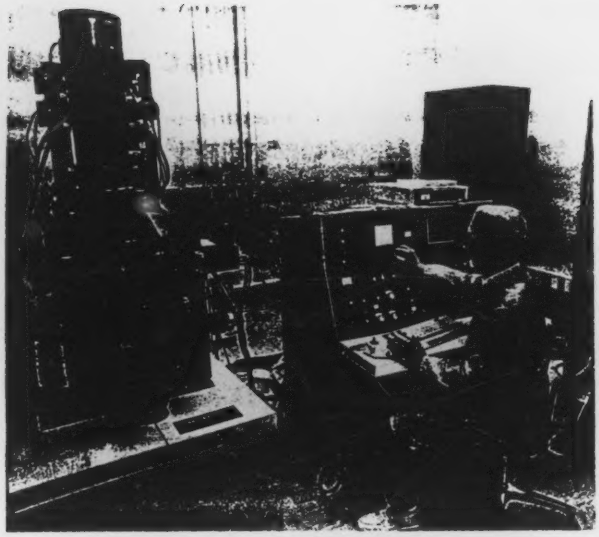


Submicron machining. It has become possible to fabricate devices in the submicron range in which properties of matter are altered qualitatively because of quantum effects. Brian Whitehead is shown here operating a scanning-electron-beam microfabricator in the National Research and Resource Facility for Submicron Structures at Cornell University. The machine is used in the fabrication of superconducting tunneling junctions having an area of 10^{-10} cm^2. Robert A. Buhrman and his colleagues have used such junctions to measure nonequilibrium electron effects in one-dimensional geometries and to study macroscopic quantum tunneling. (Courtesy of Robert A. Buhrman.)

scientific opportunities unfold, and are flexible enough to move rapidly in new directions. Independence is the hallmark of this flexibility, and the research could reasonably be described as "independent group research."

Progress in small physics

A comprehensive description of small-scale research would require summarizing a large portion of contemporary physics; such a task would require volumes. However, it is possible to convey the nature of at least some of this research by sketching a few of the advances.

Small physics progresses by numerous experimental discoveries and the close interplay of experiment and theory. For example, meticulous studies of phase transitions in systems ranging from ferrites to gels have culminated in the creation of a fundamental theory of phase transitions—the renormalization-group theory. Experimenters have created new states of matter: a superfluid Fermi liquid, compounds in which electrons simultaneously occupy two valence states, one- and two-dimensional systems, new phases of liquid crystals and many others. Discoveries include the quantized Hall effect—two-dimensional quantum-mechanical phenomena manifested by real materials—and the fractional Hall effect—the signature of a totally unexpected electron gas condensate (see PHYSICS TODAY, June 1981, page 17 and July 1983, page 19). The development of techniques to create controlled disordered systems such as spin glasses has led to a new appreciation of random systems and the first clear picture of motion in random potentials. The discovery of new nonlinear phenomena, such as solitons and strange attractors, has vastly broadened our understanding of nonlinear dynamics and has found applications in topics as diverse as atmospheric turbulence, population dynamics and ventricular fibrillation.

Lasers have revolutionized spectroscopy. They have made it possible to create and study large classes of atomic and molecular systems and to witness multiphoton processes never before seen. Molecular ions, which play key roles in both interstellar chemistry and combustion, have for the first time been studied comprehensively. The flow of energy when two molecules collide can now be traced at the quantum level. New techniques for studying single electrons and ions allow measurements of the fundamental constants with unprecedented sensitivity and precision.

Small physics plays a central role in generating and elucidating fundamental physical and mathematical concepts. The renormalization-group theory, for instance, uses techniques developed in relativistic field theory. Out of this work grew the modern theory of phase transitions and fluctuations. The theory of phase transitions, in turn, is an essential ingredient for understanding the earliest stages of the universe as taken from contemporary ideas in cosmology, particle physics, and statistical mechanics. To cite another example, the concept of broken symmetry was developed independently in condensed-matter physics and particle physics. The acquisition of mass by a particle in a state of broken symmetry is manifested in superconductivity through the Meissner effect; in gauge theories it is manifested by the Higgs mechanism.

Some of the most demanding tests of quantum electrodynamics lie in extremely precise measurements in atomic physics. The interplay of experiment and theory in this area represents one of the most relentless confrontations of experiment and theory in all of physics. Quantum electrodynamics is the prototype of gauge theories; the search for its limits of validity bears on our understanding of the electroweak theory, quantum chromodynamics and other gauge theories. Other precision experiments have set limits on the isotropy of space, the possible breakdown of symmetries such as time-reversal invariance, and the validity of the principle of equivalence.

Technical as well as conceptual links join small physics to other areas of science. Magnets developed from research on high-field superconductors have made possible a new generation of accelerators. Atomic clocks are essen-

Molecular physics. Yuan T. Lee (background), Laurie Butler (left) and Alec Wodtky at Lawrence Berkeley Laboratory are shown with an apparatus used to measure the energies of the fragments of a molecule when it is dissociated by laser light. Such photofragment spectroscopy allows the study of molecular reactions starting from a moment when the reacting species are close together in a precisely defined quantum state. (Courtesy of Yuan T. Lee.)

tial in very-long-baseline radioastronomy; laser interferometers have opened the way to a new class of gravitational-wave detectors. Laser light-scattering spectroscopy is being applied to problems in physics, chemistry, biology and medicine.

These examples, which are drawn from only a few of the many active fields of small physics, illustrate the importance of discoveries in small-scale research to the advance of physics. These examples do not, however, begin to suggest the scope and rapid development of new front-line areas. Many subfields have sprung into existence or have rapidly grown into major research areas within the past decade. The list includes:

▶ *Surface physics.* A host of new techniques has opened the way to studying surfaces and surface phenomena: ultra-high-vacuum methods, tunneling microscopy, x-ray scattering from synchrotron sources, ion scattering, supersonic helium scattering and laser spectroscopy. There have been comparable theoretical advances, some resting on new numerical techniques. The research is advancing rapidly on the structure and excitation of surfaces, surface reactions and catalysis.

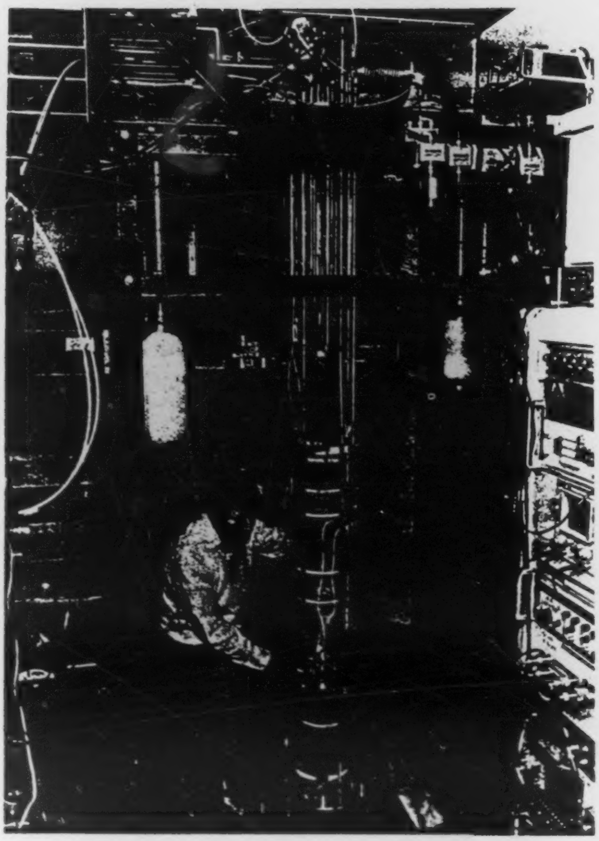
▶ *Artificially structured materials.* Molecular beam epitaxy and other techniques make it possible to create materials that are controlled on the atomic scale: superlattices composed of alternate layers of ordered crystals, amorphous layers that are disordered in two dimensions but ordered in the third, and systems that are effectively one-dimensional, such as those obtained by ion milling and other submicron machining techniques. Such materials, have properties found nowhere else in nature.

▶ *Clusters.* Using supersonic molecular beams, it is now possible to create clusters of atoms or molecules in sizes up to hundreds of particles. These clusters provide the opportunity to observe how matter organizes itself as it evolves from single particles into solids, and how atomic and molecular properties manifest themselves in condensed matter. With metal clusters we can see how band structures develop with increasing size, and we can witness catalytic and other surface phenomena.

▶ *Femtosecond spectroscopy.* Light pulses as short as 12 femtoseconds (12×10^{-15} seconds) have been generated, allowing the observation of the dynamics of molecules and solids at time scales never previously approached. In principle, one can take a "snapshot" of a molecule while it vibrates, witness how electrons in a semiconductor equilibrate or see how chemical reactions take place in solutions and in biological systems.

▶ *Nonlinear optics.* Nonlinear optical techniques have opened new areas in spectroscopy and have also made it possible to study atoms and molecular collisions in ways never before possible. Multiphoton processes, ultrasensitive detection, optical harmonic generation and laser cooling of atoms and ions are some of the advances made possible by nonlinear techniques.

▶ *Polymer physics.* The new ideas emerging from the theory of phase transitions and critical phenomena, combined with such experimental methods as laser light-scattering spectroscopy, nuclear magnetic resonance



Ultra-low-temperature physics. Dilution refrigerators have extended the frontiers of low-temperature physics into the submillikelvin region. This cryostat, shown with Douglas Osherhoff at AT&T Bell Labs, can reach temperatures of less than 100 μK. The cryostat is currently used to study the A_1 phase of superfluid He^3 and nuclear antiferromagnetism in solid He^3. (Courtesy of Douglas Osherhoff.)

and neutron scattering, have profoundly affected our understanding of the processes of polymerization and self-assembly of natural and synthetic macromolecules. These systems, consisting of polymers, micelles, macroemulsions and liquid crystals, are of central importance in many industrial, biological and physiological processes.

This "sampler" should give some idea of the great breadth and rapid progress in small-scale research. A broader portrait of these activities will be presented in the volumes of the *Physics Survey* that will be published shortly by the National Academy of Sciences and the National Research Council.

Impact on society

To appreciate the impact on society of research in small physics one need only think of what the world would be like if the transistor had never been developed. It would be a world without microprocessors, computers and the vast network of data-processing equipment on which science, governments and businesses depend, a world without instantaneous global communications and without the countless feedback and control devices that are essential to everything from jetliners to pacemakers.

Many of these developments emerged from basic research by small groups in industrial laboratories. These laboratories account for about 30% of the basic condensed-matter research in the United States. The strong coupling between industrial and university groups is one of the strengths of these research communities in the US, contributing to the advance of basic physics and the creation of new technologies.

Technology. The returns to society from small-group research can be prodigious. For example, magnetic-resonance imaging is widely regarded as the most important advance in medical diagnosis since the discovery of x rays. Magnetic-resonance imaging was made possible by research on nuclear resonance in chemical and biological systems, the development of superconducting magnets, theoretical research on tomography and image reconstruction, advances in numerical processing and high-speed data-processing devices, and, underlying it all, the discovery of nuclear magnetic resonance decades ago.

The laser has had an enormous impact on science and society. Today these devices are employed in nearly every area of physical science, as well as in the biological sciences and in medical research and therapy. Remote sensing with laser light is used to monitor pollutants, to analyze combustion and to study the atmosphere; laser metrology is used for high-precision surveying, for aligning accelerators and highways, and for controlling scientific instruments and machine tools.

The laser and modern optics play major roles in technology and industry. Fiber-optic cables are revolutionizing data transmission and long-distance communications. Laser printing has become a multibillion-dollar business. Laser-assisted manufacturing is increasingly employed in industry. Laser machining is ideally suited to robotics; it has been estimated that within a decade lasers will be involved in 30% of all the manufacturing operations in the US.

In addition to contributing to basic understanding of nature, small physics is vital to our national programs in defense, energy and the environment.

Education. Over half of the nation's PhDs in physics are trained in condensed-matter physics and atomic, molecular or optical physics. When the other small research activities are included, the fraction is over 70%. Thus the nation depends on small research groups to train most of the physicists who will carry forward basic research, execute the national programs and generate the new technologies that are essential to industry.

Students who carry out an experimental thesis in one of the independent groups usually acquire expertise in many areas. In atomic physics, for example, it is not unusual for a student to become highly skilled in mechanical and vacuum design, electronics, optics and lasers, computer hardware and software or numerical analysis; the student may also have acquired specialized skills in molecular-beam techniques, charged-particle optics or cryogenics. Other fields involve a different mix of skills, but every field of physics where a student designs, constructs and operates a piece of apparatus produces physicists with a broad range of basic skills who can move rapidly in new directions.

Priorities in big and small physics

The decision to construct a major research facility or to embark on a large research program generally requires a consensus within the pertinent research community. Typically this is achieved through debate and discussion conducted by an oversight committee or special panel. The debate may become heated, for different options often appeal to different segments of the community, but if the panel does its work well it will emerge with a prioritized list of the field's needs. These priorities define the major line of scientific development in the years to come, and they provide the funding agency with a reasoned request for the needed support. Time and again this process has helped the fields of big physics to move forward.

The process of setting priorities in small physics is less visible. Because these fields generally advance in many directions simultaneously and change rapidly in response to new opportunities, attempting to forecast the most promising new lines of research is not likely to be productive. Even a cursory look at the advances in the past decade reveals important developments that had not been—and could not have been—anticipated in the early 1970s. Thus priorities in these areas cannot be identified by a short list of topics in order of preference. Furthermore, most of these fields are supported by several agencies, sometimes without much coordination between them. There may be no "champion" agency to push vigorously for the priorities even if they could be succinctly identified.

Nevertheless, the independent research groups do set priorities. The process, though not as formal as in big physics, is just as demanding. The priorities are set by the individual researchers, who respond to intellectual challenges as new concepts emerge and new technologies become available, and by their peers, who must give their approval before most agencies will grant funds. Peer review can be a harsh arbiter, particularly when support is short.

Strong evidence for the effectiveness of this priority-setting process can be found by comparing the present "hot topics" with those of ten years ago; the list has changed significantly. The large number of new subfields—some of which were described above—provides a clear picture of how small physics sets its priorities.

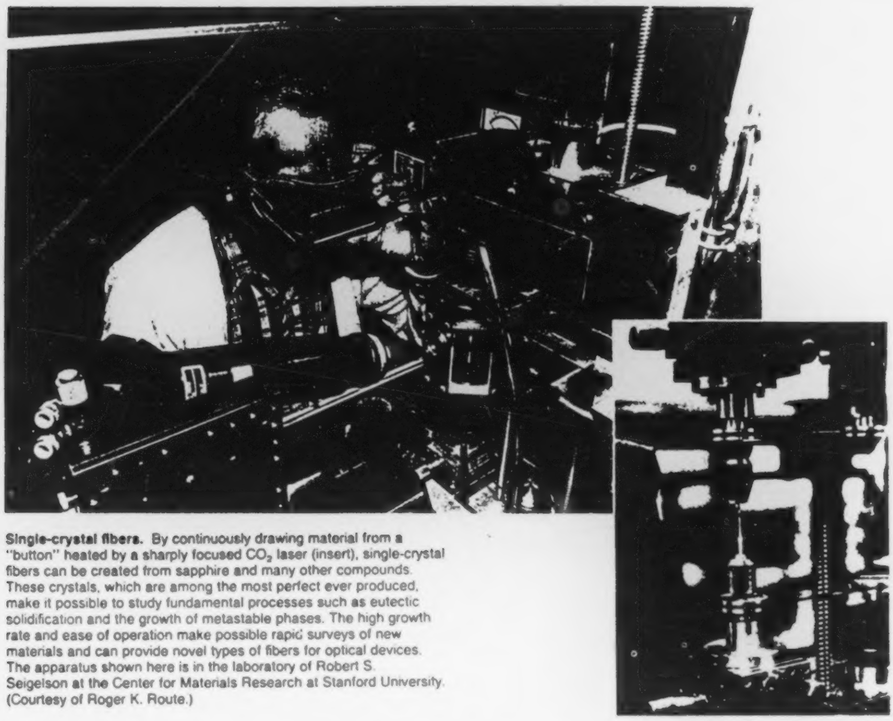
Concerns for the future

In times of cutbacks in the support of science, the small research groups can adapt rapidly to changing conditions. The scope of the research may be diminished and progress may be retarded, but one is not faced with the trauma of closing an existing accelerator or canceling a proposed one. Being small and flexible appears to be an advantage—but it has its dangers. One has an advantage in dealing with short-term cuts in support. However, in a prolonged period of scientific austerity, as we experienced throughout the 1970s, the independent research groups are vulnerable to being slowly starved for support by having to adapt to a pattern of chronic underfunding. The problem of deteriorating support is aggravated by the fact that much of the funding is subjected to the vagaries of mission-oriented agencies, which are neither directly responsible for basic research nor accountable to the basic research community. There is no mechanism for assuring continuity of research when an agency loses interest.

The most conspicuous evidence of serious trouble in small-scale research is the critical shortage of instrumentation in the university laboratories—a problem that has been widely noted.[2] Essential instrumentation in university laboratories is often obsolete or simply missing. The research is being seriously retarded; university groups are being excluded from new research areas. Molecular-beam epitaxy, for example, an important technique for making artificially structured materials, is pursued in many industrial laboratories but in few universities. The $1-million price tag for a molecular-beam epitaxy machine is too high for most research programs.

The DOD–University Instrumentation Program provides a perspective on the problem. In the first year of this five-year program, budgeted at $30 million per year, the requests were over $645 million. Estimates place the total need at over $1 billion.

There are other signs that the climate for small-scale research in the US



Single-crystal fibers. By continuously drawing material from a "button" heated by a sharply focused CO_2 laser (insert), single-crystal fibers can be created from sapphire and many other compounds. These crystals, which are among the most perfect ever produced, make it possible to study fundamental processes such as eutectic solidification and the growth of metastable phases. The high growth rate and ease of operation make possible rapid surveys of new materials and can provide novel types of fibers for optical devices. The apparatus shown here is in the laboratory of Robert S. Seigelson at the Center for Materials Research at Stanford University. (Courtesy of Roger K. Route.)

is deteriorating. Machine shops and electronics shops have been shut down in universities across the nation, and specialized services, such as crystal growing and materials preparation, have been terminated. Forefront research requires a healthy infrastructure of these services; their disappearance is symptomatic of chronic underfunding.

The low number of US graduate students is an additional reason for concern. Although the total of PhDs graduated each year has remained relatively constant for almost a decade, the number of US students has been falling. Forty percent of the entering students are now foreign (see PHYSICS TODAY, September, page 74). The reasons for this are complex. Most US graduate students are supported through grants to the independent research groups, while many foreign students are supported by their home governments. Furthermore, when students see that their professors are unable to purchase vital equipment, to support graduate students, postdocs, visitors and technicians, or even to travel to meetings, they will move to other fields. There can be no doubt that the low funding levels for the independent research groups are a contributing factor to the declining trend in US enrollment.

The picture of postdoctoral employment in the US today is particularly revealing; the *Physics Survey* reports that more than half of the postdoctoral researchers are foreign. These researchers, many of them supported by their own governments, are welcome guests in US research groups that lack funds to support US physicists.

The United States gains much from the foreign students and postdoctoral workers who participate in research here. Furthermore, their training often constitutes a highly cost-effective form of assistance to their countries of origin. Nevertheless, the relatively small number of young US physicists is obviously a source of concern.

There are other underlying problems, though they are harder to quantify. Academic careers involving small-scale research are significantly less attractive today than in the past. Although the number of academic openings is not large, many universities are finding it difficult to fill them with qualified candidates. The universities have lost much of their ability to compete with industrial laboratories. Salary can be an important factor, but the decisive factor is often that the industrial laboratory can provide the resources to move the research ahead rapidly while the university cannot. The demand for academic physicists is expected to increase sharply in the 1990s. Unless the climate for support improves, there will likely be a severe shortage of qualified candidates in universities throughout the nation.

Collectively these problems are cause for deep concern over the future of small-scale research in the United States and the future of physics in our universities.

The cost of small physics

There is wide misconception about the cost of small physics. Even small equipment is expensive. An electron-beam scattering apparatus can cost \$200 000; a tunable laser system, \$150 000—and many experiments use more than one laser. A dilution refrigerator costs \$200 000; a molecular-beam epitaxy machine, as mentioned, can cost $1 million or more. Setting up a typical small laboratory can cost anywhere from a few hundred thousand to over a million dollars; if the research requires services such as materials preparation, the setup costs can be much higher.

Operating an independent research group in a university requires support for graduate students, postdoctoral workers or other research staff, technicians, materials and supplies, and support for the up-to-date equipment. Total costs vary from field to field, but an active university research group can require $200-$400 thousand per year; many groups need much more.

Most of the research support in the US comes from the Federal government. Federal grants supporting experimental research in small groups are generally well under $100 000 per year—far too little to meet the actual costs of the research. Many groups are thus forced to apply for several grants. Physicists spend valuable time searching for support, preparing proposals and juggling budgets between various grants. The time they can spend with students is being diminished; their research is slowed down. This pattern has corrosive effects in laboratories across the nation. Small research groups have lost much of their capacity to pursue new ideas vigorously and to move in new directions.

The large difference between the size of the research grants and the costs of the research threatens the future of the small independent research groups in the US. After a decade of severe winnowing, attempts to direct funding to a smaller number of groups would do as much harm as good. Funding must be brought up to a viable level. Federal concern for basic research within the past few years has provided a favorable climate for addressing these issues. Recent sharp increases in support for some areas of basic physics are encouraging, but the overall situation for the small research groups continues to deteriorate.

A perspective on small physics

The many areas of research in small groups comprise a conglomeration of activities supported by different agencies for different purposes. There is no obvious way to judge the nation's need for any single portion of this research in the context of the total support for physics. It would be difficult, for instance, to attempt to balance the nation's need for low-temperature physics against the nation's need for a new accelerator.

However, if small-group research is viewed collectively, it assumes a clear identity in the world of physics and in the national scene. The small research groups produce much of our best science, and they train the majority of our graduate students; in addition, they generate much of our new technology. Thus these small groups constitute an invaluable resource for the nation.

One can obtain a reasonably clear picture of the national investment in small-scale physics by examining the support for research in the universities. In 1983 the Federal government is estimated[3] to have spent approximately $330 million in the universities for "research and development" in physics. This figure essentially represents support for basic research. Of this amount, approximately $280 million is estimated to be used for research by small groups.

Estimates on the need for support of small physics, based on material gathered by the Physics Survey Committee for its forthcoming report, appear to point to the same conclusion: For the research to move ahead effectively, this support needs to be roughly doubled over a period of perhaps four years. This would require that the nation increase the total investment in these areas by $70 million per year—in real dollars—for four years. In addition, a special infusion of support for instrumentation in university laboratories is needed urgently.

When one views small-scale research collectively and witnesses its enormous impact on science and its abundant returns to society, this research must surely represent one of the most cost-effective ways for the nation to invest in research. The cost of assuring the continued vitality of this research is modest compared to the total cost of modern physics; it is extremely small compared to the national expenditures for research and development based on the fruits of this research.

In the process of deciding the future direction of physics in the United States, the collective needs of the small independent research groups deserve to be considered in parallel with the needs for new accelerators and other major facilities.

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Much of the information in this article is based on material gathered by the Physics Survey Committee of the National Academy of Sciences. The author thanks the members of the Physics Survey Committee, and particularly the chairman, W. F. Brinkman. He also acknowledges helpful discussions with G. B. Benedek, R. J. Birgeneau, A. M. Clogston, C. P. Flynn, W. A. Fowler, T. J. Greytak, G. Holton, V. Jaccarino, M. A. Kastner, J. D. Litster, P. C. Martin, D. E. Pritchard, N. F. Ramsey and D. C. Shapero.

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2. *Revitalizing Laboratory Instrumentation*, Office of Physical Sciences, NRC, National Academy P., Washington, D.C. (1982).
3. *National Patterns of Science and Technology Resources, 1984*, NSF 84-311, 1984. The estimate of $330 million for 1983 is based on the reported value of $306 million for Federally funded R & D expenditures in physics at universities and colleges in 1982. □

Senator **Ford**. And we do thank both of you for being here today very much.

Senator **Melcher**. Mr. Chairman.

Senator **Ford**. Oh, excuse me, John. You slipped in on me. Will you take it? I have to go to the floor.

Senator **Melcher** [presiding]. All right.

Dr. Tigner, have we heard—at least I have not heard testimony today about what is necessary in terms of geological structure in burying the magnet.

Dr. **Tigner**. Well, I'm not exactly sure how to address your question, but let me start and then you can interrupt me if I'm not getting your way.

We of course were most concerned that we should understand what the basic requirements would be for the foundations for such an accelerator. So we made a very detailed study of the geographical, geological, and topographical distributions which one finds within the United States.

And we discovered that, by careful categorizing them, we could find—we could make three different conceptual designs which would—based upon the most prevalent of these distributions, three different conceptual designs, each of which would be equally satisfactory from the technical point of view of the SSC.

One of them was a site in which it's suitable to make excavations from the surface. Another one of these generic sites, which fills a lot of the—or would fit in a lot of different places in the United States, are ones in which a medium depth tunnel, something like 50 to 70 feet deep, could be bored through mixed soil with some rock.

And we also took the circumstance for a complete conceptual design in which one would have to go so deep that you could get into solid rock.

We went through an elaborate cost analysis of each of these types of sites and determined that in fact the overall cost of putting the SSC in these three different, very different foundation situations, if you will, were more or less the same, and that the differences in the costs would be much more governed by local peculiarities, rather than the generic type of geological structure that was there.

Now, it's obvious that you don't want to have earthquakes and volcanoes going off at you all the time. On the other hand, this kind of technology is really very forgiving in terms of—by "this kind of technology" I mean the accelerator technology"—really rather forgiving in terms of very small vibrations or slow sags and so forth.

So it's much easier than you might think to find a place that's satisfactory to support it.

Senator **Melcher**. In other words, it does not need to be, first of all, 60 to 70 feet deep?

Dr. **Tigner**. There has to to be a certain minimum amount of cover on it in order to provide the necessary temperature stability and to shield against the possibility that the beam might escape. But after that, there's any depth.

Senator **Melcher**. Well, all right. Let me start again. Does it need to be 60 feet deep?

Dr. **Tigner**. If you're trying to get into the site parameters or the invitation for site proposals and ask me to comment about the numbers therein, I think I should back off and say that that's a Department of Energy document. We gave them our best technical advice about how to describe the necessary siting for the SSC, and there it is.

And I could not disagree with their choice of parameters.

Senator **Melcher**. Well, I guess I want to get an answer one way or the other that I can understand, without making a career out of it.

You said it could be in a number of places in the country. I would have to think that there is some governing force on how deep it is, for one reason or another.

Dr. **Tigner**. Oh, if you were to show me a site, then I could make a cost estimate of how much it would cost to build the machine on that site, taking account of all the local peculiarities and problems you might run into.

Senator **Melcher**. Well, let's start with, is the water table important also?

Dr. **Tigner**. Certainly the water conditions are important.

Senator **Melcher**. And what does that mean, that it should not be moist or there could be a certain amount of water?

Dr. **Tigner**. We certainly could take care of a certain amount of water by engineering precautions.

Senator **Melcher**. In other words, draining the fill or pump it out if there is too much water?

Dr. **Tigner**. Or seal against it.

Senator **Melcher**. Yes.

Then could it be in the desert, California desert?

Dr. **Tigner**. It could certainly—desert is not a very precise designation. You could certainly—

Senator **Melcher**. California desert is a pretty precise designation.

Dr. **Tigner**. Well, there is a marvelous variety there. You certainly could put this thing in a desert. It might cost you more to carry away the used heat. It might cost you more to get the water there that you needed. It might cost you more to get the electricity there.

But you certainly could operate it.

Senator **Melcher**. The depth, then, is relative to the temperature of the soil? And the atmosphere, of course, that would not make much difference.

Dr. **Tigner**. No, I think the depth that you would pick at a given site would be more appropriately picked by the geological conditions. It would be much more cost effective to go to a depth at which the material was rather homogeneous, so that you could use one construction method. So that would be probably the governing factor that would set the depth at a given site.

Senator **Melcher**. In other words, the depth and the type of soil and the geological structure is not so important for the scientific success of the SSC; it is simply a question of construction costs?

Dr. **Tigner**. Absolutely.

Senator **Melcher**. Thank you very much.

I want to thank you both, Dr. Tigner and Dr. Weinberg, for your testimony. I was not here to hear you, Dr. Tigner, but I was able to hear most of Dr. Weinberg's, and I did read both of your statements presented here. And I appreciate it very much.

I feel that what you are leading us toward is one of the more important matters that has come before this Committee since I first became a member, some 10 years ago.

Thank you very much.

Dr. **Tigner**. Thank you, sir.

Dr. **Weinberg**. Thank you.

Senator **Melcher**. The Subcommittee is adjourned.

[Whereupon, at 4:40 p.m., the hearing was adjourned.]

APPENDIXES

APPENDIX I

Responses to Additional Committee Questions

Follow-up Questions from Senator Ford

ANSWERS TO POST HEARING QUESTIONS RELATING
TO THE APRIL 7, 1987 HEARING BEFORE THE
SUBCOMMITTEE ON ENERGY RESEARCH AND
DEVELOPMENT OF THE SENATE COMMITTEE ON
ENERGY AND NATURAL RESOURCES

WITNESS: DR. ALVIN W. TRIVELPIECE
DIRECTOR, OFFICE OF ENERGY RESEARCH
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Question: You stated at the hearing that "something like a third of our gross national product probably owes its existence to our knowledge of those sub-atomic parts of the atom, and a good fraction of that is knowledge that has been gained in this area." Please document this claim. Are you implying that 1/3 of our GNP is due to high-energy physics? What part of the GNP is due to chemistry? What part to condensed-matter physics? What part to biology?

Answer: When I made that statement I was attempting to relate to the Committee that past investments in basic research have repaid themselves many times over in applications useful to society. While I was talking about the SSC at the time, my statement was made in the broader context of high energy physics, nuclear physics, atomic physics and related fields. While I do not have hard documentary evidence that one-third of our GNP is derived from such discoveries, it is my considered judgment that this estimate is reasonable. Just to name a few, the nuclear medicine and nuclear power industries owe their existence to basic physics research performed in the past. I do not have estimates available for other disciplines.

Question: Isn't it misleading to claim that high-energy physics is responsible for contributions to wealth that require other branches of science? Should we conclude that $1 invested in high energy physics is more productive than $1 invested in other areas of physics? Can you document such a claim? Please discuss.

Answer: I have not attempted to make comparative judgments among competing fields of science regarding their impact on the GNP. I am simply trying to point out that investments in long-term basic research, such as knowledge of the atom and its subatomic parts, have substantial subsequent impact on the nation's GNP. I was trying to make the point that a significant portion of our GNP owes its existence to advances in fundamental understanding of the atom which have arisen earlier this century. It is certainly true that the path from the fundamental understanding provided by high energy physics to the specific contributions in today's GNP often involves contributions from other science and engineering disciplines.

Question: Please describe the options available to the Administration, as you said at the hearing, to ensure that funding the SSC "is not going to be done within the already existing budget envelope which the Department has."

Answer: The Secretary brought the SSC directly to the attention of the Domestic Policy Council and the President as a policy item. The SSC has been approved by the President as a science initiative along with his proposal to double the funding of the National Science Foundation over the next five years. Future years' funding for SSC will have to be considered in the context of the overall broad base of the Federal budget. It is not possible to identify specific puts and takes.

Question: Please explain how you interpret the site selection guidelines to preserve the chance for a relatively poor state to prevail over a rich state that offers a large bonus for the SSC if each State contains acceptable sites of more or less equal technical merit.

Answer: The selection process should preclude a bidding war among states. The goal of the SSC site selection process is to select a site that will permit the highest level of research productivity and overall effectiveness of the SSC facility at a reasonable cost of construction and operation and with minimal adverse impact on the environment. This is reflected in the relative emphasis between technical evaluation criteria and cost considerations contained in the Invitation:

> "Although cost considerations are significant, primary emphasis will be placed on the results from the evaluation of the technical evaluation criteria by the NAS/NAE in the development of their BQL (best qualified list) recommendation to the DOE. DOE will place similar emphasis in its determination of the preferred site."

Furthermore, the cost considerations focus on life cycle costs which include construction, operation, and environmental mitigation costs as well as contributions. For example, a site where the cost of labor is high may result in a less favorable life cycle cost relative to a site where the cost of labor is much lower - even if a substantial contribution is provided by the proposer.

DOE will select the preferred site based upon an evaluation of proposals received. Auctioneering will be precluded because modifications bettering the initial proposals will not be accepted.

Given these considerations, we believe all potential proposers have a chance of being selected.

Question: Add up, for the following basic science facilities, the cost between now and 1996 of construction of:

- the SSC;
- improvements at the Stanford Linear Collider;
- improvements at the Tevatron in Illinois;
- the 1-2 GeV Light Source at Berkeley, California;
- the Continuous Electron Beam Accelerator Facility in Virginia;
- the Compact Ignition Tokamak at Princeton;
- the Advanced Photon Source at Argonne National Laboratory;
- the Relativistic Heavy Ion Collider at Brookhaven National Laboratory; and
- the Advanced Neutron Source and Oak Ridge National Laboratory

Answer: The cost between now and 1996 of construction for the basic science facilities you asked about is $7.2 billion (in as spent dollars).

Question: Add to that the proposed increases in the cost of operation for this period of existing DOE programs in basic science and the proposed programs of the National Science Foundation.

Answer: The proposed increases for existing DOE Office of Energy Research programs in basic science for this period are estimated to be well below a half a billion dollars (in as spent dollars).

The proposed increases for National Science Foundation programs for this period are estimated to be about $5.5 billion (in as spent dollars).

Question: How much do all these increases add to?

Answer: The above increases for the basic science facilities, the existing Office of Energy Research basic science programs, and for the National Science Foundation programs add to about $13.0 billion (in as spent dollars).

Question: Is anything left out?

Answer: The above list accounts for all known potential increases including a number of facilities (Advanced Photon Source, Relativistic Heavy Ion Collider, and the Advanced Neutron Source) whose construction starting dates have not yet been proposed.

Question: Does the Administration intend that these increases come at the expense of other programs? Which ones?

Answer: The Administration has proposed that funding for the National Science Foundation and the SSC not be at the expense of other science programs but that funds be made available from the broad base of the overall Federal budget.

Question: If not, then won't the deficit have to increase to accommodate this new emphasis on basic science?

Answer: The Administration is committed to deficit reduction and believes the resources for these programs must be identified within the overall Federal budget.

Question: The last Congress approved construction of the Continuous Electron Beam Accelerator Facility in Newport News, Virginia. Under the plan we approved it was anticipated that CEBAF would require about $60 million in FY 1988. Yet the FY 1988 budget only requests a little over $30 million for CEBAF, and now the project will be stretched out: finished later than scheduled at higher total cost.

Why was CEBAF cut in the President's budget?

Answer: The earlier anticipated FY 1988 funding level for CEBAF could not be accommodated within resources available in the FY 1988 President's Budget Request. Due to the strong commitment to deficit reduction a balance was arrived at between the specific funding for this project and other important projects and programs.

Question: Isn't this a very practical example, at a lower level of expenditure, of how SSC could be treated after it is approved?

Answer: I prefer not to speculate on the future, however, a guaranteed future funding level does not seem likely for any project or activity in the budget.

Question: Wouldn't delays of this type mean higher total costs? CEBAF's total cost will increase as a result of this cut, won't it?

. Answer: Stretch out beyond a normal schedule of construction funding may result in higher total costs for the particular project and this must be balanced against the overall budget and fiscal considerations.

Question: Would you agree that this is a likely scenario for the SSC?

Answer: It is difficult for me to judge at this time what actions future Administration's and the Congresses may take. I hope the Administration and the Congress can cooperate to bring this project to fruition in a timely and cost effective manner.

Question: In Sunday's Washington Post (April 5, 1987) there was an article on the new superconductors that said, among other things, that with superconductors that operate at liquid air temperatures,

- "Enormous particle accelerators, such as the planned 52-mile, $4.4 billion Superconducting Super Collider, which use magnets to propel protons at nearly the speed of light, could be built at greatly reduced costs and on much smaller scales".

Do you agree?

Answer: No. Even if the many technical obstacles were overcome, the Super Collider could not be built at greatly reduced costs on a much smaller scale. Possible cost savings by eliminating the very low temperature needed for the present design are about 5 percent of total project cost. Even if materials existed today with the necessary parameters, it would be irresponsible to risk the reliability of the project without a lengthy and costly R&D program to develop and prove out the new magnet design. The net savings would at best be problematical. Economically, the present design field is already very close to optimal. Increasing the field would make the project smaller in circumference, but the increased amounts of superconducting materials and steel to contain the forces of an increased field more than offset the modest gains in tunnel costs (which are only about 10 percent of the project cost).

Nonetheless, we have been following the dramatic developments in superconducting materials with great interest. If suitable materials can be found, they will be used for upgrades of the Super Collider, probably starting with increased event rates by replacing magnets near the interactions with magnets having greater focusing power. It should be realized, however, that accelerator magnets unfortunately represent a particularly demanding application of superconductivity. They require very high current carrying capacity and must be capable of being drawn into fine filaments to a fraction the diameter of a human hair. They must be wound and twisted into strong, yet flexible cable that can withstand the strong forces that are inevitable when the magnets are energized. The new materials have very poor current carrying capacity and degrade badly in magnetic fields. They are also very brittle. Large scale applications of these new materials to accelerator magnets will unfortunately be well in the future--a minimum of 10 to 20 years, if ever. Many substantial new inventions will be needed.

Applications to other technologies will be more direct, and the recent advances in the development of these new superconducting materials are exciting and worthy of intense exploration. In fact, Department of Energy laboratories are actively engaged in this research and have been at the forefront of a number of recent advances. For the reasons stated above, however, application to the SSC will be difficult, with no certainty of the many technological advances needed for the SSC. We therefore believe strongly that there is no basis to delay the SSC.

Question: How should we react to these new developments?

Answer: We should pursue R&D to understand the nature, performance, and limitations of these new materials. There are potential near term applications in many areas of great significance such as instrumentation and cryogenic computers, and there are possible long-term applications to accelerator magnets which could be valuable in 10 to 20 years. The Department is pursuing an aggressive R&D program to investigate these promising new materials.

Question: For example, should the design of the SSC take these new superconductors into account? How?

Answer: As I indicated earlier, I do not feel that there is any evidence at this time to indicate that these materials can meet the needs of the SSC magnets nor to support modifying the design of SSC or delaying its construction.

Question: If not, why not?

Answer: Because of the present fundamental limitations in the properties of the new materials for this application, the many engineering and technical developments required to manufacture usable wire and develop designs for higher field magnets if the fundamental limitations were to be overcome, the cost uncertainties, and the likely 10 to 20 year period to develop usable accelerator magnets if all these difficulties were somehow to be overcome.

Question: I understand that research is being conducted on ways to accelerate particles using lasers and large electric fields in plasmas. These techniques wouldn't require large circular tracks, but would use linear facilities. The particles being accelerated would, in effect, "surf" electromagnetic waves in the plasma.

If these techniques become available, won't a 52-mile long synchrotron like the SSC seem an anomaly?

Please comment?

Answer: Current R&D is pursuing a number of advanced technologies which show promise of someday producing very high gradient linear accelerators. These devices, of great interest because of possible future use in linear electron-positron colliders that would operate at center-of-mass energies greater than 1 TeV, include laser-plasma accelerators, inverse free electron laser accelerators, inverse Cerenkov accelerators, advanced radiofrequency accelerators, and particle-plasma accelerators.

Before proposing the SSC for consideration for construction, the possible impact of the new accelerator technologies on the SSC was studied in detail by a special subpanel of the High Energy Physics Advisory Panel (HEPAP) and by the JASON group. The result of the HEPAP study, presented in the Report of the HEPAP Subpanel on Advanced Accelerator R&D and the SSC (DOE/ER-0255, December 1985), concludes that while the field of advanced accelerator research is flourishing with innovative ideas, most of which are applicable to the future possibility of multi-TeV electron-positron linear colliders capable of investigating processes involving the mass ranges accessible to the SSC, the state of development of this research is such that it does not offer an alternative to the SSC. Further, at least 10 years of development are needed for the advanced accelerator concept studies to evolve into designs for practical, engineered devices. In the subpanel's opinion, the earliest time at which an electron-positron linear collider, if feasible, could overlap the range of physical interest of the SSC is 15 to

20 years. At the present time, there is no guarantee that this can be done successfully, and there is no persuasive evidence that a linear collider using any of the new technologies could be built in the future at a cost less than that of the SSC.

The review by the JASON group reached a conclusion totally consistent with that of the HEPAP Subpanel.

Question: Your proposed schedule for siting the SSC has been criticized as giving too much advantage to the states like Texas or Illinois that have been active in seeking the SSC already to the detriment of other states that might have trouble getting a proposal together by this August.

How do you react to this criticism? Shouldn't the schedule be stretched out so that everyone has a chance to bid?

Answer: The information required of proposers, to a large extent, can be based on readily available data. We are not requiring, and do not expect, costly and time consuming environmental and geological investigations or analyses as part of their proposal. Only those proposals included on the best qualified list will be asked for such additional site data.

The conceptual design for the SSC is complete. Proposers have not been requested to provide any engineering and design data for the SSC itself.

Given the notice of the information required in the Invitation for Site Proposals, we believe that a completely responsive proposal can be prepared in the time allowed.

ANSWERS TO POST HEARING QUESTIONS RELATING TO THE APRIL 7, 1987 HEARING BEFORE THE SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT OF THE SENATE COMMITTEE ON ENERGY AND NATURAL RESOURCES

WITNESS: DR. ALVIN W. TRIVELPIECE
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Question: The Department just last week issued its Invitation for Site Proposals. These proposals are due by August, which is 5 short months away. Does this contracted schedule put some States at an unfair advantage: that is, will those States that have already spent a great deal of time and money preparing for this event have a decided advantage over States that are just now becoming aware of the project? Wouldn't postponing the August deadline put all interested States on a more level playing field?

Answer: The Department of Energy (DOE) feels that the time available for the preparation of Superconducting Super Collider (SSC) site proposals was adequate to ensure an equal opportunity for all proposers to prepare responsive proposals. Information requested from proposers was carefully narrowed to only that essential to evaluate sites for a project of this magnitude. Furthermore, all states had received draft criteria and it was commonly known years ago that there would probably be competition for the site.

Further, the proposal preparation period was extended 30 days to September 2, 1987, to incorporate any changes proposers felt necessary to respond to Amendment No. 2 to the Invitation for Site Proposals for the SSC. This amendment conformed the Invitation to legislation that prohibited the Department from considering financial or other incentives offered by site proposers in the selection of a site for the SSC.

Question: With GAO's recent criticisms of DOE's cost estimates on its accelerator activities, how are we to believe that the SSC cost estimates are any more accurate than DOE's other estimates?

Answer: The SSC cost estimate is supported by an extensive four-year R&D program and is carefully documented in the 1984 Reference Designs Report and the 1986 Conceptual Design Report. Both of the studies were carefully reviewed in depth by the Department. These reviews included the Department`s Office of Energy Research and the Department's Independent Cost Estimating staff, as well as extensive use of outside experts. These reviews concluded that the Conceptual Design Report provides a reasonable estimate and a sound basis for proceeding with the project. We recognize, of course, that there are site dependent costs which will not be known until the site is selected.

Question: Many people would like to see our physics research base within universities resuscitated from its current, fragile state. What could the SSC contribute to the strengthening of our academic physics research base?

Answer: The SSC represents the next major HEP facility in the U.S. Since university scientists perform over 3/4 of all the high energy physics experimental research in this country, the SSC capability and potential for entirely new discoveries will permit U.S. university researchers to participate in research at the frontier of this forefront research field. This is a most important aspect of strengthening the academic research base and has been recognized as such by the research community.

Question: How much federal money currently goes to supporting this research? (according to the Kleppner testimony, the answer is approximately $330 million per year).

Answer: There are two major mechanisms by which high energy physics research funding is provided to universities. One is by way of direct funding of university contacts and grants and the second is indirect funding by way of the funding provided for facilities at the national accelerator laboratories, which are utilized primarily by university research groups. FY 1988 direct funding to universities for high energy physics is estimated to be about $100 million, including both the DOE and NSF support. Indirect funding, via facilities and capabilities provided at national laboratories, is estimated at about $300 million based upon 3/4 of the research performed by university scientists. Thus, about $400 million is provided in total funding for university research for particle physics.

Question: Could these universities benefit as much from the SSC as they could if $4.4 billion were applied directly to their individual research programs over the next 10 years?

Answer: The Department feels the SSC would benefit them as much as if the money went into individual research programs. While a large infusion of Federal funding would certainly have many benefits for the universities, the quality of the research they perform is critically dependent on having facilities with world-leading research capabilities. The SSC is a major endeavor that would strengthen our academic physics research base. The purpose of large centralized facilities, such as the SSC, is to provide scientists with forefront research capabilities which can serve university research needs. The SSC is an end product of a planning process in which the university academic community has participated strongly. Without facilities such as the SSC the U.S. universities, which currently perform some 3/4 of the research on such machines, would not be able to conduct world forefront research in particle physics in the U.S., and therefore, would not be competitive with scientists in other parts of the world such as CERN-Western Europe, the USSR, and Germany where major new facilities are being constructed for new high energy physics research programs.

Question: What portion of the SSC's overall costs are likely to be assumed by international collaborators?

Answer: The Department will seek cost sharing for the SSC with all interested countries. We have already had preliminary informal discussions with some of the prospective foreign participants in the SSC. Based on these discussions, we believe that the prospects for international participation and cost sharing are excellent. We are now developing plans for formal discussions through which we will actively pursue opportunities for international participation and cost sharing. It is too early to make a definitive prediction of what portion of SSC costs are likely from foreign participants but I am optimistic that such contributions will be substantial. I would like to include for the record a DOE fact sheet, International Collaboration in High Energy Physics, which gives additional background information on this subject.

(The information follows:)

U.S. DEPARTMENT OF ENERGY
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WASHINGTON, DC 20585

DOEFACTS:

International Collaboration in High Energy Physics

The Department of Energy will seek cost sharing for the Superconducting Super Collider with all interested countries. Such cost sharing could take the form of in-kind contributions such as magnets or detectors. International interest in the possibility of contributing to the super collider has been expressed specifically in the on-going discussions with the seven other Nations of the Economic Summit. It also has historical precedents.

Historical Precedents

- High energy physics has benefitted from a long history of cooperation throughout the worldwide science community. It is recognized that no major project in high energy physics research is done exclusively by individuals from any one nation.

- It has been the tradition of the accelerator centers around the world that their unique facilities, limited in number, must remain open to all scientists, and that the selection of their projects is based upon the scientific merit of a research proposal, regardless of its origin.

- The remarkable progress in recent high energy physics research has required large collaborative teams of scientists. Currently, American scientists are part of such teams fabricating detectors for US research projects selected by the European Organization for Nuclear Research Center (CERN) in Switzerland, the Deutsches Electronen Synchrotron (DESY) Laboratory in in Germany and the Institute for High Energy Physics (KEK) in Japan. The collider detector (CDF) at the Fermi National Accelerator Laboratory is a joint effort of the U.S. with Japan and Italy.

Discussions with Economic Summit Nations

- President Reagan and the other leaders of the Economic Summit Nations recognized in their 1982 Declaration at Versailles that science and technology can contribute to economic growth and employment and at Williamsburg in 1983, they committed their countries to cooperation in specific areas one of which was high energy physics.

- For more than three years since the 1983 commitment, the United States Department of Energy has led an Economic Summit Working Group in discussions designed to find ways to avoid duplication of major facilities and costly equipment for high energy physics research. The members of the Working Group are high level officials appointed by the governments of the Economic Summit Nations: the United States, Canada, France, Germany, Italy, Japan, the United Kingdom and the European Communities.

(MORE)

- A statement from the Summary Conclusions of the Economic Summit Working Group meeting in 1983 concerning the super collider is significant:

  > "Participation of other countries in the construction of (this) accelerator could be envisaged. As is the usual practice in high energy physics, it is anticipated that scientific research would be open to qualified scientists from all countries, with the different countries contributing to the cost of the experimental detectors."

Question: I notice that "human genome mapping" is another area that DOE intends to actively pursue, at an estimated cost of perhaps $3 billion. How would you rate the relative merits and payoffs of this "genome" project in comparison to the SSC?

Answer: The SSC and the human genome both address fundamental questions, but the areas they explore are quite different and the anticipated benefits are long term. I don't know how to rate one project relative to another in a different field of research assuming both are viewed as critical. We have carefully reviewed both projects and have concluded that each is extremely important both within its own field and within the Department's overall basic science effort. I hesitate to speculate on the relative importance of the benefits of these projects which are well off in the future and where, in both cases, the most profound discoveries are likely to be totally unanticipated today. It should be pointed out that the human genome initiative proposed by the Department of Energy is focused on providing the technical resources needed to characterize the genome in the most economical manner possible. By developing techniques and instruments which can process genetic material much faster than is currently possible, the cost of the effort should be significantly less than some of the early estimates associated with this project, such as the $3 billion estimate you mention in your question.

Question: When considered against other Federal R&D program initiatives (for example, NSF funding increases, global environment R&D, SDI research, NASA space station programs), how would you rate the SSC in terms of near-term benefits to society?

Answer: I'll do my best to respond, Senator, although I'm sure you recognize that I am far less knowledgeable about some of the programs you mention than about the supercollider.

I consider both the SSC and the other programs you mention to be very important to this country, and I most strongly endorse proceeding in a balanced way toward all of these goals. They have very attractive common features: frontier science; advanced technology; and are major symbols of America's ability, prestige, and commitment to the future.

Their relative scientific merit and near-term benefits are difficult to compare and necessarily speculative. I think, for example, that most of us can more readily imagine the likely tangible consequences of the space station. I think that the public's image of mankind's long-term future in space, as portrayed in very popular motion pictures and television programs, makes it much easier for us to visualize and relate to the activities that a space station would make possible.

The supercollider, on the other hand, represents a search for a less tangible but much more basic commodity - new knowledge and understanding of matter and energy on the most fundamental level. To appreciate the importance to society of fundamental new information, we must be reminded of the enormous benefit and widespread application of earlier great research discoveries such as electro-magnetism, relativity, and nuclear processes. From that point of view, it is strikingly obvious that new knowledge of how nature behaves translates, increasingly quickly these days, into new ways of controlling natural phenomena to great benefit for society.

In terms of science and technological spinoff, I think there are qualitative differences between many of the programs you mention and the supercollider. I would not characterize supercollider physics as better than the scientific advances the space station may provide, but I think most scientists would agree that it is more fundamental. As such, the potential for technological spinoffs is probably greater over the long haul, but also much more difficult to characterize since new discoveries and future new knowledge are, by definition, not "know-able."

Senator, I recognize that you and your colleagues in the Congress must address very difficult questions such as you are asking of me, and I am making every effort to be as responsive as possible. I think it would be tragic for the future of our country if we should conclude that we could not, as a Nation, afford to proceed with all of these programs, including the supercollider.

Question: Could you elaborate on the criteria that will be considered in the site selection process? What are the relative rankings of these criteria? Where does cost rank in the decision? Will States offering large State contributions be treated more favorably than States that have little or no State funds to contribute?

Answer: SSC site proposals will be evaluated against both technical evaluation criteria and cost considerations. There are six technical evaluation criteria, including:

- Geology and Tunneling (e.g., the suitability of the land for efficient construction and operation).
- Regional Resources (e.g., the adequacy and proximity of community resources to support the SSC).
- Environment (e.g., the significance of environmental impacts from the SSC).
- Setting (e.g., the availability of land and the ability to provide title on schedule).
- Regional Conditions (e.g., the absence of natural and manmade conditions that would adversely impact the SSC).
- Utilities (e.g., the availability of utilities such as electric power and water to support the SSC).

These criteria are listed in descending order of relative importance. Cost considerations should be important in the selection process and will be used only after the technical excellence of the various sites has been evaluated. At present, The Domenici Amendment makes it clear that an "auctioning - type" process among the best technical sites is no longer possible. The Department expects to work closely with the Congress, to establish a means for using state cost sharing in the final site selection process.

Question: The ERAB physics review panel recommends increasing DOE's basic physics research funding by 50%, in addition to funding the SSC. If a choice had to be made to do one or the other, but not both, which would you choose in terms of overall improvements to the health of our physics research base?

Answer: I believe that the Nation needs to pursue both of these activities. I do not believe that it is appropriate to speak simply in terms of an either/or choice between these two initiatives. For the long-term health of this Nation it is imperative that we increase our effort in basic research. This is a vital investment in our future and that of our children. A decision on these initiatives must be made in the context of an overall strategy for basic research funding. The President has indicated his intent to increase support for basic research over the next five years. Within this Administration strategy, I believe that we can find a way to pursue the SSC along with an enhanced program of basic physics research.

APPENDIX II

Additional Material Submitted for the Record

Issue Brief

Order Code IB87096

SUPERCONDUCTING SUPER COLLIDER

Updated March 18, 1987

by

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Science Policy Research Division

Congressional Research Service

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SUPERCONDUCTING SUPER COLLIDER

SUMMARY

The Reagan Administration has proposed the construction, over the next eight to nine years, of the world's largest and highest energy particle accelerator, the Superconducting Super Collider (SSC). In 1988 dollars, the project is estimated to cost about $4.4 billion and have annual operating budgets after 1996 of about $270 million. In current dollars, these costs have been estimated by some observers to be over $6 billion and $500 million respectively. The SSC is envisioned as the "next generation" high energy particle accelerator, needed to expand the frontier of particle physics research beyond the capabilities of existing machines. Although many scientists, policy analysts, and policymakers support such a project, not all do. A major concern is that other areas of science, particularly small science, would suffer if such funding is allocated to a single, large project. In addition to its scientific benefits, the SSC's supporters suggest a number of related technical, economic, and social benefits. Not all agree, however, that such benefits would occur or that they would be significant compared to costs. Because high energy physics is increasingly an international effort, international cooperation and competition also must be considered. This complex of factors makes the congressional evaluation of the relative merits of the SSC a difficult task. Because the DOE proposes to start construction of the SSC in FY89, basic funding decisions probably will have to be made during the 100th Congress.

ISSUE DEFINITION

Given the projected cost and complexity of the SSC, questions arise as to its scientific merit, its impact on other areas of science, and its economic and social costs and benefits. The basic issue is whether the Congress wishes to authorize construction of the SSC at this time, as proposed by the Administration.

BACKGROUND AND POLICY ANALYSIS

Scientific Background

High Energy Particle Physics

In the world of subatomic or "elementary particle" physics, most experimental knowledge derives from experiments conducted on large, complex, and expensive machines called particle accelerators. Since the first discoveries of nuclear physics in the 1890s, physicists have uncovered a growing array of elementary particles which constitute the building blocks of matter and the fundamental forces of nature.

The earliest model of the atom consisted of a nucleus formed by protons (particles with a positive electrical charge) and neutrons (particles with no electrical charge) surrounded by a cloud of electrons (particles with a negative electrical charge) in well-described orbits. In the last 30 years, this simple, but effective, model has been greatly complicated by the discoveries of over 100 other subatomic particles. These new elementary particles were discovered by examining the collisions of protons and electrons with each other and with other targets in particle accelerators. The "debris" of these collisions, which was formed by the conversion of energy into matter, contained these new particles, which have lives on the order of only millionths or billionths of seconds. As energies of the accelerators were increased over the years, more and more particles were discovered. The result of these experiments and parallel theoretical developments was a picture of matter that was much more complex than previously thought.

Over the last few decades, a new theory of the structure of matter-- called the Standard Model -- has evolved. This theory currently describes matter as consisting of two sets of elementary particles and a set of fundamental forces of nature. Protons and neutrons once thought to be elementary particles themselves, now are thought to be constructed of one of the new sets of particles called quarks. The electron is a member of the other set called leptons. The four fundamental forces of nature-- strong, weak, electromagnetic, and gravitational -- describe the ways matter interacts and is held together. One of the major triumphs of the Standard Model is its ability to show how two of these forces-- electromagnetic and weak -- are connected. Scientists are currently trying to extend this theory to include the strong force, thus forming a Grand Unified Theory of fundamental forces. Physicists believe that, ultimately, it may be possible to fit the gravitational force into this theory.

Elementary particle physics deals with the very smallest constituents of matter. Cosmology, the study of the creation and evolution of the universe, deals with the very largest accumulations of matter -- stars, galaxies, and intergalactic matter. These two subjects overlap when the origin of the universe is considered. In the accepted theory of the origin of the universe -- the "Big Bang" theory -- the four forces of nature were all unified at the instant of creation and all matter consisted of quarks and leptons. As the universe cooled, the forces separated and the quarks began forming a number of particles including, finally, protons and neutrons. Of the six types of leptons, only the electron remains as a stable particle in ordinary matter.

Although considerable experimental verification of these theories has occurred up through the current generation of particle accelerators, there remains much more to be verified. Certain particles have been predicted in order to satisfy various parts of the Standard Model. These particles require considerably higher energy to produce than exists in current accelerators. In addition, in order to simulate conditions closer to the actual starting point of the universe to test the hypotheses of the Big Bang theory, much higher energy accelerators are needed than now exist. While it will not be possible to produce the energies required to reach the starting point or to verify all the predictions of the Standard Model, much additional exploration can take place by building a larger accelerator. This is the reason that the high energy physics community has proposed the Superconducting Super Collider.

Particle Accelerators

Particle accelerators have evolved from the breadbox size cyclotron invented by Dr. Ernest O. Lawrence in 1930 to the proposed $4 to $6 billion SSC. This growth has been required to achieve the higher energies needed to search for new particles. A number of particle accelerators are in operation throughout the world. Some of these machines are linear accelerators and some are synchrotron (circular) accelerators. Currently, synchrotrons are used to achieve the highest energies because particles can be accelerated repeatedly, as they continuously circle through the machines. Large magnets are required to force the charged particles to move in a circular path. Linear accelerators do not require magnets since the particles travel in a straight line. The length currently required to achieve very high energies, however, is too great to make them practical for the energies needed by the SSC (about 20 trillion electron volts) unless new ways to accelerate particles are formed (see below). Some of these machines are "fixed target" machines where a beam of particles is accelerated into a fixed heavier target. Others are colliders in which two beams of particles hit head on. The world's major high energy particle accelerators, including those under construction and proposed, are listed in the following table.

TABLE 1. Operating or Under Construction

Brookhaven - Upton, NY	proton synchrotron (28GeV on fixed target)
Fermilab - Batavia, IL	proton synchrotron (900GeV on fixed target)
CERN - Geneva, Switzerland	proton synchrotron (450GeV on fixed target)
SLAC - Stanford, CA	electron-positron linear accelerator (20-50GeV on fixed target)
Cornell - Ithaca, NY	electron-positron collider (5GeV x 5GeV)
DESY - Hamburg, W. Germany	electron-positron collider (5GeV x 5GeV) (23GeV x 23 GeV)
SLAC SLC - Stanford, CA	electron-positron linear (collider) (50GeV x 50 GeV)
CERN - Geneva, Switzerland	proton-antiproton collider (330GeV x 330GeV)
Fermilab Tevatron - Batavia, IL	proton-antiproton collider (900GeV x 900GeV)
CERN LEP - Geneva, Switzerland	electron-positron collider (50GeV x 50GeV)
Tristan* - Tsukuba, Japan	electron-positron collider (35GeV x 35GeV)
DESY HERA* - Hamburg, W. Germany	electron/positron-proton collider (30GeV x 800GeV)
Inst. of Nuclear Physics - Novosibirsk, U.S.S.R.	electron-positron collider (5GeV x 5GeV)
Institute of High Energy Physics - Serpukhov, U.S.S.R.	proton synchrotron (70GeV on fixed target)
UNK Phase I*	proton synchrotron (3TeV on fixed target)
UNK Phase II *	proton-antiproton collider (3TeV x 3TeV)
	Proposed
SSC - U.S.A	proton-proton collider (20TeV x 20TeV)
CERN LEP-LHC - Geneva, Switz.	large hadron collider (5-10eV x 5-10TeV)

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* Under construction.

Notes: 1GeV = 1 billion electron volts of energy; 1TeV = 1 trillion electron volts of energy.

The Superconducting Super Collider

The SSC would be the largest and most energetic particle accelerator in the world. The SSC as envisioned currently would require an oval tunnel 52 miles in circumference approximately 30 or more feet underground. This dimension, if superimposed on a map of greater Washington, D.C., would approximate the Capital Beltway.

The tunnel would have a diameter of about 10 feet. Within it would be two pipes, each surrounded by cryogenic superconducting magnets (that is, magnets cooled to liquid helium temperature, 4.3 degrees Kelvin above absolute zero). Superconducting magnets would be used to limit the electric power requirements of the accelerator's magnetic field. Using conventional magnets would make such power costs prohibitively high. Inside each pipe would be a narrow beam of protons. These proton beams would travel in opposite directions. At several locations (interaction halls) in the tunnel, the beams would cross for collisions. Detectors would monitor the interactions which would occur at a rate of about 100 million per second. The detectors would feed data to very large computers for continuous analysis.

The SSC could be constructed with existing technology. The principal of colliding beams of protons was pioneered at the European Center for Nuclear Research (CERN) and has evolved successfully at Fermilab's Tevatron. Fermilab also successfully used cryogenic superconducting magnets in its Tevatron. Detector and computer technologies are advanced enough to make the research productive. In short, there is high confidence in the scientific community that the SSC would live up to scientific expectations.

The DOE Program

1983-1986

The SSC is proposed as a major program of the Office of Energy Research (OER) of the Department of Energy (DOE). Its inception, however, can be traced to the worldwide attention given to the concept of a superconducting super collider since the late 1960s. The views of the U.S. high energy physics community began to crystallize during a meeting of a division of the American Physical Society in Snowmass, Colorado, in 1982. At that time a consensus developed that the next needed step in the development of high energy particle accelerators would be a multi-TeV (multi-trillion electron volt) particle collider.

In 1983, the Subpanel on New Facilities of DOE's High Energy Physics Advisory Panel (HEPAP) recommended, and the full HEPAP unanimously endorsed, the immediate initiation of an SSC having beam energies between 10 and 20 TeV each. In addition, the advisory panel recommended several other actions affecting other accelerators around the country. At about that time, the Tevatron at Fermilab was commissioned and operated successfully. This pioneer facility, which uses superconducting magnets, validated the basic systems concepts needed for the SSC.

In 1983, DOE began preliminary R&D for the SSC. On November 19, 1983, the House Committee on Science and Technology held hearings on the future direction of DOE's high energy physics program and specifically the SSC. In December 1983, DOE began a "Reference Designs Study" to examine magnet and systems design options, to make technical feasibility studies, and to make first cost estimates of the SSC. The study, completed in April 1984, concluded that the SSC would be technically feasible using existing technology and engineering.

In early 1984, DOE designated the Universities Research Association (URA), a consortium of 56 U.S. research universities, to conduct the SSC research, development, and design activities prior to construction. The SSC Central Design Group (CDG), hosted by Lawrence Berkeley Laboratory, was established by the URA as the operating group to coordinate and supervise those tasks. Research and development related to the SSC have been carried out for the CDG by Lawrence Berkeley Laboratory, Brookhaven National Laboratory, Fermilab, Texas Accelerator Center, universities, and industry.

The objectives of the CDG are to accomplish the R&D necessary to delimit all the machine's and associated system's parameters in order to optimize the performance and cost; to specify in detail the requirements of a site; to prepare a complete and detailed plan of the envisioned SSC facility, including a firm cost estimate and construction schedule; to develop prototype magnets and other components; and to conduct extensive systems tests of the prototype magnets and associated cryogenic and control systems.

The major objectives for 1985 were the preparation of the "Superconducting Super Collider Parameters Document" (June 15, 1985), and the R&D effort to select magnets. Five basic types of magnets were studied by teams at Brookhaven National Laboratory, Fermilab, Lawrence Berkeley Laboratory, Texas Accelerator Center, and cooperating industries. In August 1985, the SSC Magnet Selection Advisory Panel made a unanimous recommendation for the SSC magnets which was accepted by the CDG. The construction and testing of prototype magnets began at that time.

In March 1986, the CDG published "Conceptual Design of the Superconducting Super Collider," a report requested by DOE. It discussed scientific needs of the SSC program, a technically feasible design for the SSC, a detailed cost estimate, and a construction schedule. The cost estimate then was about $4 billion (1986 dollars), including all R&D, preparation costs, and an estimate of the initial complement of detectors and computers, but excluding land acquisition costs which DOE assumes will be provided free by the State or locality. Costs are discussed in detail below.

Work continued throughout 1985 and 1986 on some technical aspects of SSC development but, from the scientific and technical standpoint, the SSC program was largely ready to proceed with site selection and construction.

1987 and Beyond

At a congressional briefing on Feb. 10, 1987, following President Reagan's approval of the SSC program on Jan. 30, 1987, DOE announced its site selection timetable:

April 1987	DOE to issue invitation for site proposals;
August 1987	DOE to receive and screen proposals;
September 1987	DOE to refer qualified proposals to the National Academy of Sciences and National Academy of Engineering for evaluation;
December 1987	The Academies to recommend to DOE the best qualified sites (an unranked list of several sites, with no minimum or maximum number of sites required);
July 1988	DOE to designate preferred site;
Mid-87-88	Safety and environmental review process, including Environmental Impact Statement in accordance with the National Environmental Protection Act; and
January 1989	Final site selection and site preparation by DOE.

The construction phase would begin in 1989 with operations currently expected to begin in about 1996 or 1997.

Current Cost Estimates

When President Reagan approved the SSC program on Jan. 30, 1987, $60 million had already been spent by DOE on R&D and design studies from 1984 through 1986. On Feb. 10, 1987, DOE announced that FY87 funding for the SSC would be $20 million and that for FY88 it would be $35 million ($10 million for construction and $25 million for R&D), all to be taken out of the already requested FY88 DOE high energy physics program budget of $556.6 million. DOE funding estimates for the 8- or 9-year preconstruction and construction program are the following:

TABLE 2. Estimated Budget Authority
(in millions of FY88 dollars)

FY88	FY89	FY90	FY91	FY92	FY93	FY94	FY95	FY96	Total
$35	348	615	675	670	691	709	447	185	$4,375M

PROJECT COST BREAKDOWN	
Construction	$3,210
R&D	274
Detectors and computers	719
Pre-operating	172
	$4,375

CONSTRUCTION COST BREAKDOWN		
Technical Components		$1,519
Magnets	(1,068)	
Cryogenics	(129)	
Other	(322)	
Conventional Facility		614
Collider Facilities (Tunnel)	(370)	
Other	(244)	
System Engineering and Design		307
Management and Support		205
Contingency		565
		$3,210

On Feb. 10, 1987, Dr. Alvin W. Trivelpiece, Director of DOE's Office of Energy Research, stated that these budget figures are accurate to within about 10%, assuming that the required land would be provided free. The SSC's annual operating budget after operations begin is estimated to be about $270 million in 1988 dollars.

Funding Considerations

The Accuracy of the SSC Cost Estimates

The cost of the SSC has been recognized as a major factor in the feasibility of its construction from the very beginning of its consideration in 1983. The DOE "Reference Designs Study" (1984) made the first cost estimates and the "Conceptual Design of the Superconducting Super Collider" (1986) refined those estimates. The latter estimate (p. 170) was $3.0 billion (FY86 dollars) for construction costs, separated into technical components, conventional facilities, systems engineering and design, management and support, and contingency. In the budget estimate discussed at DOE's congressional briefing on Feb. 10, 1987, and set forth above, each of these subcosts, in FY88 dollars, showed an increase, total construction costs being $3.2 billion. To this were added costs for R&D, detectors and computers, and pre-operating costs to bring the current DOE estimate up to $4.375 billion (FY88 dollars).

Three recent reports of the General Accounting Office (GAO) deal with the increasing costs of DOE's particle accelerators and problems associated with accurately estimating the costs of those accelerators (Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics, Sept. 16, 1980; DOE Physics Accelerators: Their Costs and Benefits, April 1985; Nuclear Science: Information on DOE Accelerators Should be Better Disclosed in the Budget, April 1986). The last report (p. 38) estimates the cost of the SSC, including detectors, to be about $4.9 billion (FY85 dollars). Taken together, these three GAO report's indicate the continuing concerns of GAO and the Congress with DOE's accelerator costs and the extent and accuracy of the information provided to Congress for its budgetary and oversight functions.

Relationship of the SSC to Other Federal F&D Funding

The Federal R&D Budget. Under President Reagan, the Federal R&D budget has shown steady real growth. In the attempts of Congress and the Administration to deal effectively with large deficits, R&D funding may decline in the years ahead, although no firm indications of such a decline have yet emerged.

The Federal R&D budget is vulnerable to low or no growth in the years ahead because it represents a fairly significant percentage of discretionary or controllable budget items, that is, those budget items which the Congress can fund or not fund without changing existing law. The R&D budget (approximately $66.8 billion in obligations including defense R&D) is about 13% of the discretionary budget and about 22% of the controllable outlays. As such, the overall R&D budget may represent a prime target for deficit reduction in the years ahead. Any large, newly proposed science and technology projects, of which the SSC is only one, may exacerbate the vulnerability of Federal R&D budgets.

The SSC and Civilian R&D. The Federal R&D budget for 1988, including funding for R&D facilities, is estimated to be about $66.8 billion (in obligations) of which $48 billion (72%) is for defense R&D and $18.8 billion (28%) is for civilian R&D. Assuming that the average annual SSC funding from FY87 through FY96 is about $542 million the SSC by itself, on the average, would represent about 2.5% of the Federal civilian R&D budget. If the rest of DOE's high energy physics program also were of this order of magnitude, as the pre-FY88 trend suggests, total DOE high energy physics program funding might approach 5% of the total Federal civilian R&D budget. It is likely, however, that with the initiation of the SSC there would be a substantial reduction in the non-SSC portion of the high energy physics program of DOE.

Other New and Large Federal R&D Programs. Several other major Federal R&D programs in addition to the SSC recently have been initiated or proposed. As a point of comparison in terms of costs only, five of these are presented here.

The NASA space station program was initiated in FY85. It will take until 1996 to complete. The U.S. costs are now estimated to be over $16 billion (1984 dollars) with the operating costs from about 1996 through 2016 of about $1 billion per year. DOE, along with the National Institutes of Health and some private organizations, recently have proposed to "sequence" or "map" the human genome. If started in FY88, sequencing would take perhaps ten years to complete and could cost as much as $3 billion (1986 dollars). The National Science Foundation (NSF) has a FY88 proposed budget of about $1.7 billion. The Administration has proposed a doubling of the NSF budget by FY92. If this were to occur, about $10 billion (1987 dollars) would be added to NSF's existing funding level over the next 10 years. A U.S. program to study changes in the global environment over a 10-year period has been proposed. It might begin in FY90 and extend through 2000. Current cost estimates for the entire program are $1.5 to $2 billion (FY86 dollars). The Strategic Defense Initiative (SDI), first identifiable in the FY85 budget, is largely a packaging of existing defense-related science and technology programs. The Reagan Administration plan is now to fund the first stage at $20 billion (current dollars) over 6 years.

Such large R&D programs have been and will be subject to the same types of programmatic and budgetary scrutiny that the SSC will face. The point here, however, is that a number of recently initiated or proposed large Federal R&D programs, having time scales and funding magnitudes similar to those of the SSC, will be competing for congressional attention and budget dollars in this and the next several Congresses.

Advanced Accelerator Technologies

There is considerable research underway into new ways to accelerate particles to the very high energies needed to continue investigation of elementary particle physics. These methods involve, for example, the use of very intense electric fields that can be created in plasmas and lasers. Such methods would not require the particles to circle in order to gain large amounts of energy, so accelerators built using such concepts would be linear, and could be smaller and possibly cheaper, than equivalent synchrotron machines. In addition, radiation losses resulting when charged particles move in circular orbits limit the ultimate size of a synchrotron. The SSC would be about as large a machine as could be built using synchtrotron principles. Future, larger machines probably would have to be linear for these reasons. Continued research into new accelerator methods is essential for the long-term future of elementary particle physics.

Because these new concepts are at the earliest stages of development, they are unlikely to be able to substitute for the SSC if it is desired that such a machine be built much before the end of the century. Some have argued, however, that the pace of elementary particle physics be slowed because, among other reasons, of the possibility of new accelerator techniques. In 1985, the Kendrew report on high energy particle physics in the United Kingdom concluded that,

> in view of the high cost of the research, and the possibility of new techniques being developed for accelerating particles, it would not be counter to the long-term interests of the field if the pace could be reduced worldwide and not merely at CERN. (British Particle Physicists Reject Proposed Cuts for CERN, Physics Today, v. 38, Sept. 1985, p. 69.)

Costs and Benefits

There are few absolute measures of the scientific, technological, economic, and social benefits and costs of large research programs because the consequences of basic research results are often difficult to track. Consequently, the following can describe, only briefly and qualitatively, the costs and benefits associated with the SSC. Such costs and benefits ultimately will be weighed in the political forum vis-a-vis other governmental policies and programs affecting the Nation's welfare.

Scientific

There is a consensus among U.S. high energy physicists that the SSC is the needed next step in high energy physics research. Among all U.S. scientists, however, a consensus about the potential value of the SSC's contribution to science in general does not appear to exist. What is good for one branch of science often is good for science as a whole, because of scientific cross-fertilization and the support of each other's budgets for mutual self-interest. At present, however, there is increasing concern about the allocation of limited governmental funds for science. As discussed above, the Federal R&D budget may be vulnerable to reduction. This and the initiation of one or more new, large Federal R&D programs could adversely affect U.S. science in general, and small science programs in particular, and result in lost opportunities in a number of areas of U.S. science.

Technical and Economic

High energy physics is a premier example of pure or fundamental science and basic research. Although economic benefits may accrue, the target of such research is almost always advancement of knowledge. Recently, Dr. Trivelpiece, at the DOE congressional briefing on the SSC, stated that one-third of the U.S. gross national product is based on the knowledge gained from scientific study of subatomic particles, including nuclear and high energy physics research. In particular, the electronics and computer industries are based on fundamental research in the areas of electronic phenomena and condensed matter physics and owe much of their understanding to research on subatomic particles. As valid as such a statement may be, its converse -- that most fundamental scientific research results in economic benefits -- is another issue entirely. Not every fundamental science program has resulted in significant technological and economic benefits. A recent analysis of research funding by OTA concludes that:

> Economists have shown a strong positive correlation between research and development (R&D) spending and economic growth. They have estimated private returns in excess of 20% per year and social returns in excess of 40% on private sector R&D expenditures. They have not been able to show comparable returns, and at times been unable to show any returns, on Federal R&D expenditures, except for some applied research programs in agriculture, aeronautics, and energy designed to improve industrial productivity. (Emphases in original.)

About direct economic benefits from research, the report states that the "principal benefit of research, especially basic research, is new and often unexpected knowledge, which cannot be assigned a direct economic value." (U.S. Congress. Office of Technology Assessment. Research Funding as an Investment: Can We Measure the Returns?--A Technical Memorandum. Washington, U.S. Govt. Print. Off., Apr. 1986. p. 3 and 4.)

Many of those competing for the SSC site hope that it will attract other high technology firms like the Route 128 phenomena around Boston-Cambridge, Massachusetts, and Silicon Valley in California. Many analysts of the relationships between basic research, applied research, and development, however, question the value and extent of immediate technological spinoffs as contrasted to longer term economic benefits derived from fundamental scientific research. The SSC also is being promoted by some as a method to help improve the Nation's international technological and economic competitiveness, and science education. It is similarly debatable whether the SSC will be able to contribute much to these goals. For example, many representatives of academia believe that the best way to improve the Nation's science education is to directly fund new scientific facilities and equipment in the Nation's research colleges and universities rather than a large facility like the SSC.

In addition to any direct economic and technological benefits from scientific research, technological developments related to the development and construction of experimental research facilities also can occur.

Although the SSC will use mature technologies that were pioneered elsewhere, it will use them on an unprecedented scale. Expected technological developments from such experimentation include large-scale production of superconducting materials and cryogenic refrigeration, improved tunneling technologies, large-volume storage of helium, and computer control and large-scale mechanical alignment systems. (Quigg, Chris and Roy F. Schwitters. Elementary Particle Physics and the Superconducting Super Collider. Science, v. 231, Mar. 28, 1986. p. 1525.) One cannot expect, of course, that the SSC would be the only source of such technological developments or, indeed, that such developments would have significant economic consequences. Recently, in a number of industrial and academic laboratories unassociated with high energy particle accelerator development, materials have been discovered which become superconducting at much higher temperatures than possible with current technology. (Dagani, Ron. Superconductivity Seen at Record High Temperatures in Metal Oxides. Chemical and Engineering News, v. 65, Feb. 2, 1987. p. 29-30; Robinson, Arthur L. Record High-Temperature Superconductors Claimed. Science, v. 235, Jan. 20,. 1987. p. 531-533.) If these materials can be produced with properties that permit application to superconducting magnets -- that is, as wire that can be formed into coils -- the costs of magnet systems could drop dramatically because of lessened refrigeration requirements. It likely will be many years before these new materials reach the stage of commercial applications. Thus, while potentially of great economic significance, they will not affect immediate decisions about the SSC.

The question of the economic benefits from high energy particle accelerators has not been analyzed to any extent in the United States. In Europe, however, two studies have shown that every franc spent by CERN on associated high technology development has produced about three francs of new business for the firms involved. These technologies include elecronics, optics, and computers; electrical equipment; vacuum cryogenics and superconductivity; steel and welding; and precision machining.

In summary, history has shown that many widespread economic benefits, have resulted from some fundamental scientific research. European analyses have shown that specific commercial benefits flowed to the firms involved in the construction and operation of CERN's high energy particle accelerator development. On the other hand, many observers hold that possible secondary spinoffs are uncertain at best, and should not be used to justify the primary purpose of a basic science program. Thus, while the SSC may provide future economic benefits, equivalent funds spent directly to obtain such possible economic benefits might be more effective.

Only one economic aspect of SSC development seems reasonably assured. The locality that is chosen to be the site of the SSC will benefit. Such communities do run the risk, however, of substantial economic loss should the project be cancelled once construction is underway. Local industry will be called upon to a large extent to construct the "bricks and mortar" part of the 52-mile tunnel and the required research facilities. DOE estimates that the on-site work force would reach about 4,500 people over the 8- or 9-year construction period and that the full-time staff of the SSC facility would be about 2,500, with about 500 visiting scientists expected on-site at any given time.

Social

The SSC also may generate broad social, including cultural, benefits, although such benefits may be offset by associated social and cultural costs. Some proponents of the SSC have been very enthusiastic about the cultural aspects of the SSC. One, for example, has stated that "the supercollider is critically important to the 2,500-year search for the nature of fundamental matter" (Lederman, Leon M. To Understand the Universe. Issues in Science and Technology, v. 1, Summer 1985. p. 56) and that

> ...most people acknowledge that the most important aspect of science is the cultural one. The need is universal--to give a coherent account of the world and our place in it. (Lederman, Leon M. The Value of Fundamental Science. Scientific American, v. 251, Nov. 1984. p. 40.)

Mankind almost certainly will continue to pursue this search and the SSC may help in reaching that goal. The next insights now being sought, however, are so complex that they are quite remote from most people. It may be asked, therefore, whether we need to continue the current pace of these investigations or whether we can slow down and delay construction of the SSC or its future equivalent for several years. In addition to the possibility of answering some of the great questions about nature and broadening the Nation's intellectual horizons, the SSC may contribute to the Nation's overall scientific prestige and probably would be a source of a number of future Nobel Prizes.

While these and other perspectives of the social and cultural value of big science in general and the SSC in particular are legitimate, many members of U.S. society may place greater immediate value on promoting, for example, health, education, welfare, housing, urban development, and other policies and programs that are more directly related to the social development of the Nation's people than on science per se or even on the economic developments derived from that science. It should be remembered, though, that these programs are not necessarily mutually exclusive.

International Cooperation and Competition

The Administration has proposed that the SSC be located in the United States and constructed mainly with U.S. funds, although international cooperation has been welcomed informally. Such international cooperation might involve the procurement of some system components, like some superconducting magnets, from foreign sources, and certainly the conduct of joint research projects, involving joint project funding, once the SSC is operational. Formal international cooperative agreements would be made through normal diplomatic channels.

The high energy physics community traditionally has been international and cooperative. The increasing costs of high energy particle accelerators and the numbers of smaller machines that have been or will be decommissioned will reinforce this tradition. CERN, at which many U.S. high energy physicists have worked, is operated by an European consortium and the HERA collider at DESY in West Germany has components supplied by a number of European countries. The SSC is a prime candidate for such international cost sharing.

There are both benefits and difficulties, however, associated with international cooperation in big science, including high energy facilities and operations. Some of these benefits and opportunities are: making greater resources available in terms of information, knowledge, and know-how necessary for any scientific activity; making possible a wider range of topics and a broader range of approaches; reducing the financial burden on all participants; speeding up the entire innovation process, from basic research to application; reducing wasteful redundancy; and enhancing good will and communication among the participants. Some of the costs and difficulties are: inherent difficulties in meshing disparate national bureaucracies; delays in reaching decisions among differing political and legal systems; complications in varying decision processes, priorities, and competencies; costs of international bureaucracy; the danger that political inertia, which makes projects hard to start, but even harder to stop, will dominate; the possibility of drains on national research budgets because of international commitments; the tendency to undertake, internationally, only low-priority projects; and the apparent conflict between cooperation and improving a Nation's competitive position. (Rycroft, Robert W. International Cooperation in Science Policy: The U.S. Role in Macroprojects. Technology in Society, v. 5, 1985. p. 51-68. Also published as International Cooperation in Science: the U.S. Role in Megaprojects. In Emerging Issues in Science and Technology, in 1982. Washington, National Science Foundation, 1983. p. 1-13.)

Although not all of these factors would apply to the SSC, it is clear that international cooperation in big science may not be considered to be a unmitigated good from a national standpoint in every case. A decision by DOE, for example, to enter into agreements with Japan or another country to supply superconducting magnets may be met with disfavor by U.S. industry which has cooperated with DOE in the development of such magnets.

Although high energy physics is characterized by international cooperation, it also is characterized by international competition. The titles of two recent articles in the scientific literature express this forcefully: "European Physicists Push Alternative to SSC: (Science, v. 228, May 24, 1985, p.968-970) and "The Shifting Balance of Power in Experimental Particle Physics" (Physics Today, v.39, Nov. 1986, p.27-34).

An European large hadron collider (LHC) was once considered to be an alternative to the SSC. The LHC is an idea of Carlo Rubbia, the physicist who won a Nobel Prize for his team's discovery, at CERN, of three particles predicted by the Standard Model. The LHC would use CERN's existing LEP facility and consequently could be constructed much faster and more cheaply than the SSC. It would, however, have an energy of only about one-third that of the SSC and only about one-tenth of the number of particle interactions. Rubbia himself, consequently, has stated recently that, because of such characteristics, an LHC would not, in fact, be a scientific alternative to the SSC. (U.S. Congress. House. Committee on Science and Technology. Status and Plans of the United States and CERN High Energy Physics Programs and the Superconducting Super Collider (SSC). Hearings, Nov. 29, 1985, p.43.) But, should the SSC not be funded, current European accelerator plans suggest that a "favorable European option" would be a proton collider at LEP having energies of 8.5 TeV each, "targeted at essentially the same physics as the SSC." (Physics News in 1986. Physics Today, v. 40, Jan. 1987, p.S-29.) This option, however, would involve extensive development of magnets.

In addition to the Europeans, the Soviets and the Japanese have large particle accelerators under construction. The Soviet UNK Phase II collider would have a collision energy in the range of the LHC.

In evaluating the Nation's need for the SSC, opportunities for cooperation as well as challenges from abroad will be considered. National scientific prestige will be placed on the side of the scales called competition just as the need for cost sharing will be placed on the side of cooperation.

CONGRESSIONAL HEARINGS, REPORTS, AND DOCUMENTS

U.S. Congress. House. Committee on Science and Technology. Subcommittee on Energy Development and Applications. High energy physics facilities. Hearing, 98th Congress, 1st session, Nov. 19, 1983. Washington, U.S. Govt. Print. Off., 1984. 654 p.

----- Status and plans of the United States and CERN. High energy physics programs and the Superconducting Collider (SSC). Hearing, 99th Congress, 1st session, Nov. 29, 1985. Washington, U.S. Govt. Print. Off., 1986. 1557 p.

----- Task Force on Science Policy. International cooperation in big science: high energy physics. Hearing, 99th Congress, 1st session, Apr. 25, 1985. (Science Policy Study-Hearings, v. 4). Washington, U.S. Govt. Print. Off., 1986. 1096 p.

Source: DOE

THE SUPERCONDUCTING SUPER COLLIDER (SSC)

What is the SSC?

- A basic research tool in high energy physics, the field which studies the fundamental nature of matter and energy.

- The world's largest particle accelerator; a proton-proton collider with an energy of 20 trillion electron volts (TeV) per beam, twenty times the energy of the Tevatron, the highest energy accelerator in the world. (A flashlight battery has 1.5 volts).

- It will accelerate two beams of protons in opposite directions to nearly the speed of light and bring them into collision at four points where detectors will observe the results of the collisions.

- The heart of the SSC is two rings of superconducting magnets located in a 52-mile circumference, race-track shaped tunnel that is 10 feet in cross-section diameter and 30 feet or more underground.

- Other facilities include four large interaction halls where experiments will be conducted, a series of injector accelerators (comparable in size to the Fermilab accelerator in Illinois) and technical support buildings and offices.

Why build it?

- The SSC will continue mankind's search for the fundamental nature of matter and energy. Scientists have made great progress in understanding the ultimate building blocks of matter and the basic forces which govern the transformations of matter and energy. However, important questions remain unanswered. The SSC will help answer these questions.

- Continued progress in high energy physics research in the mid-1990's requires the study of collisions at energy levels that cannot be achieved with any accelerator now in operation or under construction.

- A new, higher energy accelerator is essential; the SSC is such a facility.

- Basic science has always led to discoveries and innovations that profoundly affect our lives.

- Advances in basic knowledge contribute to the economic and technological competitiveness of the nation through applications of discoveries and new knowledge and through spinoffs.
- High Energy Physics involves the use and development of the most advanced technologies. Frontier science stimulates frontier technology; there are numerous instances of the impact of high energy physics in the areas of nuclear medicine, electronics, superconductivity, cryogenics, etc.
- The SSC will inspire young students to pursue careers in science and technology.
- SSC will ensure a world leadership position for the U.S. in this important field of basic science into the next century.

How much will it cost?

- In the fiscal years 1984-1987, $76 million has been spent on R&D and design studies.
- Construction cost for the accelerator and laboratory is $3.2 billion (FY 1988 dollars); costs of R&D, detectors, computers and pre-operating activities are $1.2 billion (FY 1988 dollars) for a total of $4.4 billion.
- Annual budget after operation begins is estimated to be $270 million in FY 1988 dollars.
- Land is assumed to be provided free of charge by site proposers.
- International collaboration and cost sharing is being sought; several indications of interest; formal negotiations await U.S. Government commitment to the project.

When will the SSC be built?

- If authorized in FY 1988, the facility could be completed in 1996.

Who are the key players involved in the SSC?

- DOE provides about 90% of the Federal support for high energy physics and will be the responsible Government agency.
- DOE will select a contractor to manage the construction and operation of SSC.
- The R&D and design studies in progress at DOE laboratories, universities, and industry are being carried out for DOE under the management of the SSC Central Design Group formed by Universities Research Association, Inc., a consortium of 56 universities.
- Construction of conventional facilities and fabrication of technical components will be carried out by subcontractors and industry to the maximum extent possible.

How many people will be involved with the SSC?

- The onsite work force during construction will peak at about 4,500 people.
- Once the facility is operating, the SSC staff will be about 2,500 and about 500 visiting scientists are expected to be on-site at any given time.

A SUPERCOLLIDER CHRONOLOGY

July 1982	The SSC is conceived at a meeting of physicists in Snowmass, Colorado
July 1983	DOE's High Energy Physics Advisory Panel recommends SSC R&D as highest priority
October 1983	Preliminary R&D for SSC begins
December 1983	Reference Designs Study (RDS) begins
March 1984	DOE selects URA to manage SSC R&D and design studies
April 1984	RDS completed
May 1984	DOE reviews the RDS with favorable conclusions
December 1984	Site selection procedure to be followed is announced
June 1985	CDG's preliminary site criteria document delivered to DOE
September 1985	Superconducting magnet type selected
March 1986	CDG submits SSC Conceptual Design Report to DOE
April-June 1986	DOE reviews the Conceptual Design Report with favorable conclusions -- report demonstrates technical feasibility and provides a reliable cost estimate
January 1987	President Reagan calls for SSC's construction

Perspective

The case for the super collider

The enormous recent progress in particle physics is directly related to the power of accelerators. The next stage requires a superconducting super collider, a project the author believes the U.S. government should support.

by James W. Cronin

PHYSICISTS engaged in the study of the fundamental constituents of matter and their interactions have recently proposed the construction of a particle accelerator which will cost about $4 billion. Particle accelerators are the essential tools that enable physicists to discover the most elementary forms of matter and the nature of their interactions. The new instrument, called the Superconducting Super Collider (SSC), will be required if further progress is to be made in this most fundamental field of science.

This proposal raises questions of national science policy because of the large expense of the project. Among such questions are the following: How would the instrument benefit the people of this nation? Is it even the business of the U.S. government to support such a project? Among many large scientific projects why choose this particular one? What would be the impact on other scientific fields if such an instrument were built? Should it be built by an international collaboration? How can one be sure anything important will be learned with this instrument?

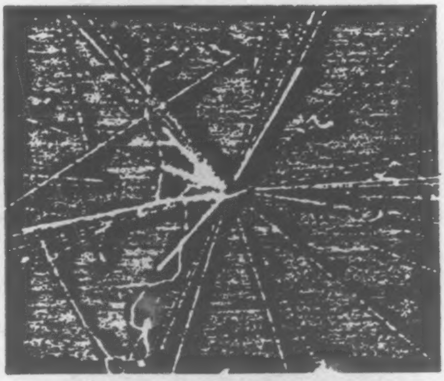
The SSC proposal is the result of the enormous growth in our understanding of the fundamental constituents of matter. This study was resumed 40 years ago after World War II by many of the physicists who had participated in the Manhattan Project. At that time the fundamental constituents of matter were thought to be the neutron and the proton, which comprised the atomic nucleus, and the electrons which formed the outer shells of the atom. Other particles were observed in cosmic radiation, but the role they played was not understood until particle accelerators were used to produce them directly in the laboratory by collisions of the accelerated protons or electrons with stationary targets. Today we know that the neutron and proton are complex structures comprised of apparently more fundamental constituents called quarks.

Forty years ago there appeared to be four basic forces: the strong force, which bound the neutrons and protons into atomic nuclei; the electromagnetic force, which bound the electrons to the atomic nucleus to form the atoms; the weak force, which is responsible for radioactivity; and the gravitational force.

We now know that the weak and electromagnetic forces have a common source. We understand the nature of the strong force and suspect that it has a common origin with the weak and electromagnetic forces. We understand the important role that the fundamental constituents and their interactions have played in the evolution of the universe following the "big bang." Finally, at a deeply theoretical level, very compelling ideas are being developed which suggest a common origin to all four forces.

It is in the context of these successes and the new questions they raise that the SSC proposal has been made. Most of our current understanding about the fundamental nature of matter has come from the use of particle accelerators of ever-increasing energy. In recent years accelerator builders have developed techniques in which two counter-rotating beams of charged particles can be brought into collision with one another. Such collisions make much more effective use of energy than do collisions with stationary targets. As the energy of the particle accelerators has increased, so has their expense.

At present in the United States there are four major high-energy accelerator centers (see table). The total capital investment in these facilities is in excess of $2 billion, and

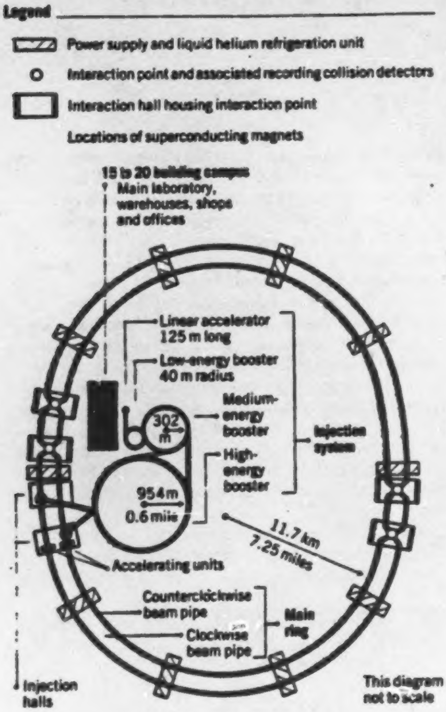


This image from the "streamer chamber" of the CERN collider in Geneva shows streams of subparticles bursting out from a collision between a proton and an antiproton. A proton collision at the proposed SSC would generate at least five times as many particles as the CERN appartus. The photograph was taken in a collision detector during CERN's UA-5 experiment, conducted by an international consortium of physicists from Bonn, Brussels, Cambridge (England), CERN, and Stockholm. *(Courtesy James W. Cronin and John Rushbrooke)*

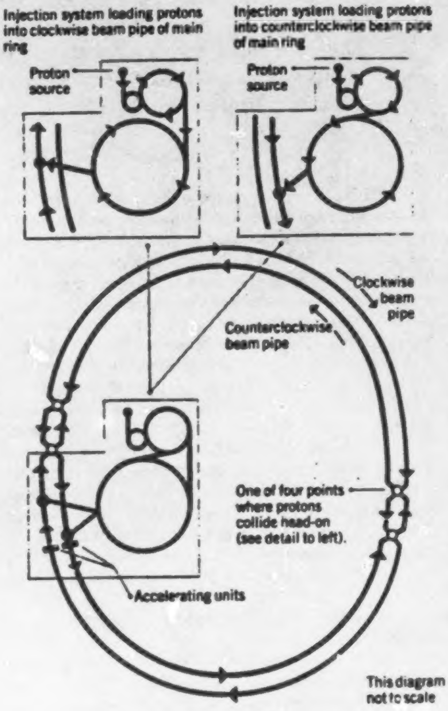
James W. Cronin, University Professor of Physics at the University of Chicago, received the Nobel Prize in physics in 1980.

The Superconducting Super Collider (SSC)

SSC anatomy (For scale drawing, see map at bottom right.)


Legend
▨ Power supply and liquid helium refrigeration unit
○ Interaction point and associated recording collision detectors
☐ Interaction hall housing interaction point
Locations of superconducting magnets
15 to 20 building campus
Main laboratory, warehouses, shops and offices
Linear accelerator 125 m long
Low-energy booster 40 m radius
302 m
Medium-energy booster
High-energy booster
Injection system
954 m
0.6 mile
11.7 km
7.25 miles
Accelerating units
Counterclockwise beam pipe
Clockwise beam pipe
Main ring
Injection halls
This diagram not to scale

Operation of injection system

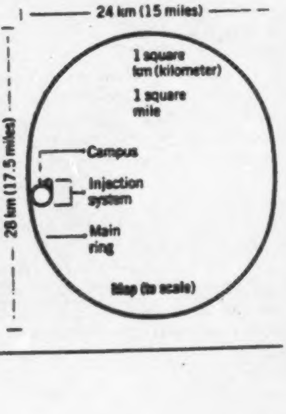

Injection system loading protons into clockwise beam pipe of main ring
Injection system loading protons into counterclockwise beam pipe of main ring
Proton source
Proton source
Clockwise beam pipe
Counterclockwise beam pipe
One of four points where protons collide head-on (see detail to left).
Accelerating units
This diagram not to scale

Main ring sustaining dual proton beams

The size and power of the underground SSC would allow narrow beams of protons to collide at almost the speed of light, creating new subatomic particles observable only at very high energies that cannot be attained by existing accelerators. This diagram tracks the path of proton beams. Protons are produced by the ionization of hydrogen atoms.

The injection system (upper right), composed of a linear accelerator and three progressively larger circular energy boosters, prepares the protons for the main-ring collisions by accelerating them to higher and higher energies. Some protons are loaded into the clockwise beam pipe (green), then others into the counterclockwise one (black).

Inside the main ring (see central schematics), accelerating units speed the protons to 20 times their energy. The protons are guided in the high-energy booster and main ring by about 10,000 powerful superconducting magnets, refrigerated by liquid helium to 4.35° Kelvin (about −270° centigrade, −455° Fahrenheit). The magnets maintain the beams on their circular paths; special magnets near the interaction points force collisions between protons traveling in opposite directions. Detection apparatuses at each collision site will measure the energy released by the collisions and will trace the paths of particles produced by the collisions. By creating levels of energy similar to those of the "big bang," scientists hope to learn about the fundamental laws of nature that guided the creation of the universe.


24 km (15 miles)
28 km (17.5 miles)
1 square km (kilometer)
1 square mile
Campus
Injection system
Main ring
Map (to scale)

International graphics: Michael Yaroff the annual operating budget is about $600 million per year. A larger level of activity exists in Western Europe, as well as a comparable effort in the Soviet Union. And Japan has significantly increased its investment in high-energy physics.

The SSC would consist of two concentric magnetic guide fields for protons.* The magnets would be placed in a 60-mile circumference tunnel bored about 30 meters below the surface. The guide fields would intersect at a number of points to allow the counter-rotating beams of protons to collide with energies 20 times greater than now available. Detectors surrounding the collision point would study the result of the interactions.

Coincident with the scientific need for the SSC has been the development and successful implementation of superconducting magnets for accelerator guide fields. These magnets can produce a guide field three times larger than a conventional iron electromagnet. This means that the circumference of the SSC can be one-third the size of an accelerator built with iron magnets. In addition, the electrical current flows in the superconducting windings of the magnets without dissipation of power. Power does have to be supplied for refrigeration since the superconducting property occurs only at liquid helium temperatures (four degrees above absolute zero). This power is only a fraction of what would have been required without superconductivity.

The combination of the scientific imperative for the SSC with the technical feasibility of its construction makes this project the prime goal of the high-energy physicists. Yet the huge cost of the project inevitably attracts national attention.

WHY SHOULD THIS expensive scientific project go ahead? It is difficult to argue that there are any immediate benefits to be felt by the whole population. It should proceed for the simple reason that the exploration and understanding of nature have consistently advanced civilization and are one of its prime features. Discoveries made in the course of fundamental scientific investigations have in time led to new technologies which have profoundly affected life on this planet. One can point to the development of electricity as a power source at the end of the nineteenth century, which was based on discoveries made in the eighteenth and early nineteenth centuries. The development of fast computers can be traced back to investigations of the fundamental properties of solid state materials which were suggested by the discovery of quantum phenomena in the early twentieth century.

I personally believe there are even deeper reasons to support the construction of the SSC. The intellectual achievements of humanity in its relatively brief time on earth are almost beyond belief. Furthermore, they are among the most positive aspects of human nature. The spirit of a nation and the pride of its people can only be enhanced when science, including the exploration of our planet, solar system, galaxy, and universe is among its highest priorities.

The United States is a strong nation, with the intellectual and economic resources to execute major scientific projects. These projects should emphasize the gain of basic knowledge without regard to immediate return on investment. Experience from the past amply documents the long-term benefits. Such idealistic goals are not without practical consequences. A higher priority for fundamental science will naturally improve the level of education. Young people with normal curiosity will be encouraged to take science more seriously. The pool of people prepared to enter a technologically dominated society will be enlarged.

Even in a nation that places a high priority on fundamen-

The world's major high-energy accelerators

(GeV: Giga electron volt, an energy unit used to measure the mass of subatomic particles. With $E=mc^2$, the mass of the proton is about one GeV.)

Proposed superconducting supercollider (United States)

20,000 GeV protons colliding with 20,000 GeV protons (1996)

United States

Fermi National Accelerator Laboratory, Batavia, Illinois

- 900 GeV protons on a stationary target
- 900 GeV protons colliding with 900 GeV antiprotons

Stanford Linear Accelerator Center, Stanford, California

- 15 GeV electrons colliding with 15 GeV positrons
- 50 GeV electrons colliding with 50 GeV positrons (1987)

Brookhaven National Laboratory, Upton, New York

- 28 GeV protons on a stationary target

Cornell University, Ithaca, New York

- 5 GeV electrons colliding with 5 GeV positrons

Western Europe

European Organization for Nuclear Research [CERN], Geneva Switzerland

- 450 GeV protons on stationary targets
- 330 GeV protons colliding with 330 GeV antiprotons
- 50 GeV electrons colliding with 50 GeV positrons

German Electron Synchroton Laboratory, Hamburg, West Germany

- 5 GeV electrons colliding with 5 GeV positrons
- 23 GeV electrons colliding with 23 GeV positrons
- 30 GeV electrons colliding with 800 GeV protons (1990)

Soviet Union

Institute of Nuclear Physics, Novosibirsk

- 5 GeV electrons colliding with 5 GeV positrons

Institute for High Energy Physics, Serpukhov

- 70 GeV protons on a stationary target
- 3,000 GeV protons on a stationary target (1990)
- 3,000 GeV protons colliding with 3,000 antiprotons (1995)

Japan

National Laboratory for High Energy Physics, Tsukuba-gun, Japan

- 35 GeV electrons colliding with 35 GeV positrons (1987) tal science, a project on the scale of the SSC cannot proceed without scrutiny. Aside from economic factors, the proponents of the SSC should be able to convince scientific colleagues in other disciplines of the intellectual value of the project and thereby gain their support. Moreover, we must convince our elected representatives of the value of the project in these general terms. We are probing the most fundamental building blocks of matter and the nature of the interactions among them. We have already made enormous progress and are proposing to build this large new machine because we are confident that we will learn a great deal more. The technology to build the SSC exists and has been demonstrated to work effectively.

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*Charged particles such as protons are deflected by magnetic fields. An arrangement of electromagnets surrounding an evacuated pipe can produce a magnetic guide field which maintains the protons in a circular orbit inside the pipe. Electrical impulses add energy to the particle on each orbit. As the energy of the protons increases, the strength of the magnetic guide field must be increased to keep the proton orbit within the pipe.

The size of the project—$4 billion spent over seven years of construction—is not large on the scale of government expenditures. However, because of its lack of immediate economic benefit, it is inconceivable that it would be built without government support. It has been suggested that the SSC could be built by international collaboration with Western Europe, Japan, or the Soviet Union.

Although these suggestions have some merit, in practice long delays will be required before such a collaboration can be realized. At present, both Western Europe and the Soviet Union have commitments to build new accelerators complementary to the SSC. An international agreement to establish an intercontinental laboratory and begin construction of an SSC could only occur well after 1990. If an intercontinental collaboration were to be formed, the site of the machine could be chosen outside the United States. In such a situation it may be difficult for the U.S. government to agree to pay for its share (about 50 percent) of the facility. Significant international contributions to the SSC, especially in the intellectual resources of people, will occur if the United States decides to proceed with the project on its own.

Consideration of international support is very important to scientific colleagues in other fields. While they can be convinced of the intellectual value of the SSC, they are concerned that funding by the United States alone will have an adverse impact on their own fields. In the past, as the support of particle physics has grown or receded significantly, there has been no evidence that other scientific fields have suffered, or profited. The principal criterion for support of any field of science has been its intellectual vitality.

Many critics ask how one can be sure that this new instrument will lead to new fundamental knowledge. As with any new accelerator of higher energy, there are no guarantees. Nonetheless, the single most important parameter responsible for the enormous progress in particle physics has been the energy of the accelerator. The pace of discovery has been a consequence of the steady increase in the energy of particle collisions provided by the accelerator. While previous accelerators have provided answers to the scientific questions which they were specifically designed to address, the most important discoveries have been those that were completely unanticipated. If we wish ultimately to understand the properties of the fundamental building blocks of nature, the SSC will have to be built. ☐

THE SUPERCONDUCTING SUPERCOLLIDER
and
THE FUTURE OF HIGH ENERGY PHYSICS

Peter Carruthers

Department of Physics, University of Arizona, Tucson, AZ 85721

Role of High Energy Physics in the Modern World

In recent years, new discoveries in high energy physics have yielded further refinement of our understanding of space, time, and the structure of matter. Such knowledge provides the basis of our conception of the everyday world in which we live. The transformation of that world by science and technology has achieved an unprecedented importance for key competitive issues such as economic viability and national security. No nation that wishes to provide its citizens with freedom and economic well-being dares to neglect the mobilization of its financial and intellectual resources to be in the front ranks of the scientific enterprise. The United States cannot become complacent in the face of an increasingly aggressive world that is attracting its finest minds into careers in research and technology. High energy physics is recognized internationally as the most fundamental of the sciences and, as such, is accorded the highest priority. We note the continued strong support of the European community for CERN (the European Center for Nuclear Research), not to mention other important facilities such as the HERA project in Hamburg. Despite much progress, many fundamental issues remain to be understood about elementary particles, their structure and interaction. Only refined experimentation with the most advanced accelerators can provide the answers to these questions.

Role of the SSC in the Revitalization of United States Science

The recent announcement of the approval of the SSC (Superconducting Super Collider) project in conjunction with an unprecedented commitment to double the NSF budget in the next five years is a signal to the world that the US is reasserting its traditional lead role in science. The SSC will be the premier large research instrument in the world for some time to come. It will achieve colliding beams of protons having energies of 20 TeV (tera-electron volts) each. The possibility of achieving decisive new understanding of the microstructure of matter will attract many of the most talented young American scientists into this and related fields, in addition to becoming a focal point for international collaboration. The SSC will allow the creation of forms of matter some 20 times heavier than presently accessible, as well as the exploration of the texture of space time 20 times more accurately, answering many current puzzles while hopefully raising others. It will also provide a demonstration experiment of the large-scale superconducting technology that is expected to play a crucial role in future society. Hopefully, increases in the NSF budget will expedite the repair of the crucial "small sciences" damaged by inadequate funding over the past 20 years. In addition, much needed innovations in cross-disciplinary research will be fostered by the NSF.

Educational Issues Related to the SSC

The education of scientists from all over the world in first-class research universities continues to give the US an advantage currently outweighing its relatively small population base. At present, the entire world adopts US research standards and techniques, not to mention its language. In addition, many of the best foreign students remain in the United States to pursue their careers, providing an invaluable influx of precious talent. This would cease to be the case if the US were to relinquish its lead position in such key fields of science as high energy physics. We view with concern the decreasing number of US students in physics graduate schools in recent years, an effect possibly due to the deterioration of career opportunities over the past 15 years. It is crucial to reverse this trend by the real and symbolic restoration of science to a valued place in the intellectual and political life of the country. Finally, we call attention to the fact that many of those receiving advanced degrees in high energy physics go on to establish careers in related disciplines such as computer science or engineering. The value for technological careers of the education in "basics" received by high energy physicists is well-known.

Economic Issues Related to the SSC

On the scale of traditional levels of science funding, the SSC seems expensive at $4.4B in FY87 dollars. Compared with usual socioeconomic units, this amount is only typical: we mention the similar cost of a space shuttle, a Trident submarine, or a major airport. Indeed, on the basis of long-term national security, both economic and military, the SSC would appear to be a good investment.

The real issue to be addressed by Congress and the scientific community is that the parallel development of the other sciences must not be compromised by the construction of the SSC. This principle was already announced during the presentation of the July 1983 HEPAP subcommittee report, which originally recommended the construction of the SSC. Fears of the existence of a "zero-sum game" have led to the ironic situation in which the principal opponents of the SSC are to be found in the ranks of other scientists, rather than the public or their elected representatives. Responsible members of the high energy community continue to assert that the SSC construction cannot be taken from the rest of science.

Evidence from a study of the economic impact of CERN as purchaser from its 519 associated high-technology industry suppliers during the years 1973-82 indicates that one Swiss franc spent in high technology generated three Swiss francs in "economic utility." (Economic utility is defined to be the sum of increased turnover and cost savings due to the CERN relationship.) This alone is equal to 60% of the total costs of operating CERN during these years and, further, does not take into account the multiplying effect in the rest of the commercial sector. Taking into account the multiplying effect of other jobs created by CERN, it is clear that the facility more than repays the cost of construction and operation in the 10-year time frame of the study. This result does not include new inventions inspired by the demands of accelerator technology, or the ultimate payoff in new applicable knowledge. On all these grounds, the SSC is seen to be a very good investment for the future of the nation.

Technology Spinoff from High Energy Physics

The need to achieve maximum effectiveness within a limited budget for the construction of successively more sophisticated accelerators has traditionally led to important technological innovations and unanticipated practical applications. To give but a few examples of the former, we note that the demands of efficient data analysis have been a principal driving force in computer development. In addition to the large-scale superconducting technology previously mentioned, we mention the development of high-power, high-frequency radiocavities and high vacuum systems, whose applications to radar and to varied industrial and research problems are well-known. The medical applications of accelerator physics to medical therapy are many, including the production of radioisotopes and the use of megavolt electron accelerators for cancer radiotherapy. Other important spinoffs include the development of synchrotron light sources for the study of surface phenomena of materials, the use of ion implantation in semiconductor devices, fast electronics techniques, and radiation processing of food and plastics. Frequently, the feedback to basic science from the applications has produced a creative synergism between basic science and the industrial-private sector technological enterprise.

The Future of High Energy Physics

Spurred by the US intention to build the SSC, CERN has announced its intention to convert its present project (LEP) to a "Large Hadron Collider" of 12-16 TeV energy after the completion of critical LEP experiments. This proton-antiproton facility would lack the luminosity of the planned SSC, but could with luck be running at an energy approaching the 40 TeV SSC design specifications, at a time (1995-6) which is the earliest one can hope for the SSC to be completed. This European effort to maintain their current small lead should be taken seriously as an indication of the priority held by high energy physics in Europe. The current commitment to LEP and HERA have created substantial financial strains; therefore, the obligation of the additional expense of LHC magnets and detectors would indicate an impressive determination not to be outclassed by the SSC. It is clear that time is of the essence if the US is to recapture and maintain its historical lead in world high energy physics.

Future accelerator facilities following the SSC have until recently been imagined to be nearly unaffordable, barring a breakthrough in technology. Recent discoveries of new superconducting materials suggest that just such a breakthrough is conceivable. Although the press has made much of the high critical temperatures observed, the possible relevance of these materials to high energy physics may be more dependent on securing a high critical magnetic field and large critical current density, and the ability to produce magnet wire in a practically usable form. If, as is thought, one can obtain magnetic fields an order of magnitude higher, it will be possible to upgrade the SSC by replacing current Niobium-Titanium magnets with the new ones, at minimal cost. It should not be thought that the new materials are of immediate technological use; indeed, it required 20 years to learn how to manufacture magnets from the Nb-Ti alloy. The exciting speculations made possible by the recent discoveries is that a future generation of high energy physics experiments may be possible in the same laboratory facility to be constructed in the next few years. The high energy physics community must thank its materials science colleagues for making the currently proposed SSC a still better investment than before.

Sheet Metal Workers' International Association
1750 New York Avenue Northwest
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Phone: (202) 783-5880

ERNEST S. LEE
Special Consultant
International Affairs

May 3, 1987

Senator J. Bennett Johnson
Chairman, Energy and Natural Resources Committee
United States Senate
Washington, D. C. 20510

Dear Senator Johnson:

I write to express the interest and concern of the Sheet Metal Workers' International Association General President Edward Carlough, and the members of this national union, that every and all work to be performed in connection with the construction of the Superconducting Super Collider be accomplished by the skilled workers of organized labor in the United States.

We also express our interest and concern that all materials, components, supplies, assemblies, and manufactured products which may be purchased and applied to the completion and operation of this science complex, to include the purticle accelerator, its undeground housing, research center, central offices, and all support and industrial buildings, be the product of the efforts of unionized workers of the United States.

As this nation pays dearly for the weakened manufacturing capabilities of our industrial base, much of which is now owned by corporate entities abroad, it is noted that the U.S. government and much of private enterprise contract with foreign companies (both governmental and private commercial) for the products and facilities we use. Foreign ownership of former American companies in the United States and their penchant for sourcing abroad could mean, if the Administration and the Congress permit it, that this vital project and its scientific product may well be an international prize, not the taxpayers' hope of a means to regain economic and political leadership in today's world.

Senator Johnson, organized labor is that U.S. force which translates the work of the engineers and managers into the products which for decades were the international standards of excellence. Organized labor in the United States is capable of producing the finest products in the world, bar none, with the proper architecting, design and engineering, responsible project management, and employers whose heads are not buried in sand.

- continued -

Every official charged with the proper conduct of the Super Collider project knows that organized labor performs the best work attainable, completing their tasks and responsibilities on time and on budget provided union contractors are in charge and the planning phases have been correctly executed. This was so with regard to the Manhattan Project and every other major undertaking of this nation in this century. Organized labor and the taxpayers of the nation would hope to see that long memory as well as that foresight we now need so very much as a nation.

Organized labor can concentrate the skills and numbers comprising the different crafts and trades for this work wherever it is needed. Organized labor can produce the materials and components, assemblies, parts, and supplies from those plans and with those machines which industrial management has in inventory or will prudently provide. It can be either an American project, or it can be "farmed out" as so much of American work has been treated lately.

And, finally, it is our view that the scientific product of the Super Collider must be translated into the U.S. manufactured products, processes and technology that will move the nation out of its grievous debtor status in the world and provide the higher-standard jobs and wages that can underwrite the redemption of the U.S. debt and a sound tax base.

As we discuss these matters with the Members of Congress and in the public view, certainly the leaders of organized labor and of responsible and bonafide U.S. industry will increasingly wish to have your views. I should be most pleased to have them too.

With every good wish,

Sincerely,

Ernest S. Lee

Ernest S. Lee
Special Consultant
International Affairs

ESL/s
cc: General President Carlough